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BY SIR RONALD ROSS, K.C.B.

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BY SIR RONALD ROSS, K.C.B.

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WITH MANY ILLUSTRATIONS

SECOND EDITION WITH AN ADDENDUM ON THE THEORY OF HAPPENINGS

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PREFACE

Malarial fever is perhaps the most important of human diseases. Though it is not often directly fatal, its wide prevalence in almost all warm climates produces in the aggregate an enormous amount of sickness and mortality. In India alone it has been officially estimated to cause a mean annual death-rate of five per thousand; that is, to kill every year on the average 1,130,000 persons —a population equal to that of a great city. This is more than the mortality of plague at its height or of cholera and dysentery combined. The total amount of sickness due to it is incalculable, but may be put by a rough estimate at between a quarter and a half the total sickness in many tropical countries. Often all the children and most of the adults are infected by it. Unlike many epidemic diseases it is not transient, but remains for ever in the areas which it has once invaded. It tends to abound most in the most fertile countries, and at the season most suitable for agriculture. Very malarious places cannot be prosperous: the wealthy shun them; those who remain are too sickly for hard work; and such localities often end by being deserted by all save a few miserable inhabitants. Malaria is the great enemy of the explorer, the missionary, the planter, the merchant, the farmer, the soldier, the

administrator, the villager and the poor; and has, I believe, profoundly modified the world's history by tending to render the whole of the tropics comparatively unsuitable for the full development of civilisation. It is essentially a political disease—one which affects the welfare of whole countries; and the prevention of it should therefore be an important branch of public administration. For the State as for the individual, health is the first postulate of prosperity. And prosperity should be the first object of scientific government.

Fortunately, as often remarked, malaria is of all diseases the one regarding which we possess perhaps the fullest knowledge. We know the cause of it and the manner in which it is spread. We know a specific cure for it, and several efficient methods of prevention. It is our own fault then if we do not reduce it as much as possible.

The literature of the subject dates back for more than two thousand years. To write a complete book on malaria, including references to all that has been worthily said or done in connection with it, would be a gigantic task. The history, the symptoms, the pathology and the treatment would fill several volumes; a long essay could be written on the parasites; an entomological work on their carriers; and a full account of the laws of diffusion, of the local distribution, of the preventive measures, and of the numerous preventive campaigns which have been conducted since the time of the ancients, would of itself occupy many hundreds of pages. But the task would be largely an unnecessary one, because we already possess many good books on

the medical, parasitological and entomological aspects of the subject.

Regarding prevention, however, especially in the light of the great developments which followed the discoveries of twelve years ago, there is much need for yet another work. Though certainly based upon biological knowledge, this part of the subject presents also its own problems for solution. The diffusion of disease is a theme which requires exact, and indeed mathematical analysis; and the art of controlling it belongs, not only to the medical man, the parasitologist and the entomologist, but even more to the experienced hygienist, the engineer, the administrative officer and the statesman.

On considering how best to meet the demand, I concluded that the only satisfactory way was to collect in one volume the thoughts and observations of the able men who have themselves worked in this laborious field, or who are in the best position to furnish the required information; and my warmest thanks are due to those who have given me so much of their valuable time and labour. My own part consisted merely in attempting to compile a preliminary analysis of the general problems and difficulties before us.

Some readers will perhaps be surprised when they fail to find in a book on the prevention of malaria any description either of the parasites and their carriers, or of the necessary technique. But in these days almost every medical man in the tropics, at least any one likely to be entrusted with the charge of anti-malaria work, is sure to be already familiar with these details. More-

over, the present volume should be written largely for the use of readers who are neither medical men nor zoologists, but who may be called upon to deal with the administrative side of the subject; and minute technicalities would be as useless to them as they would be superfluous to the trained biologist. I have therefore determined to limit myself to matters which will, I hope, be useful to the advanced student without being unintelligible to any educated reader — that is, to a logical consideration of the broader (and often more neglected) principles of the subject.

An historical introduction is always necessary to give coherence to ideas; and I have taken the opportunity to write as complete and accurate a one as my space and knowledge permit of. The summary of facts which follows (taken from my report on Mauritius) may be useful in propagandism; the experimental inoculations of men by means of blood drawn from patients or of infected Anophelines have been collected with care because of their fundamental importance in the modern theory of malaria; and I trust that these three first chapters will suffice to convince the busy layman that this theory has been based upon something more than mere conjecture.

In the chapters on the parasitic invasion of the individual and of the community, I have departed considerably from medical custom by laying much stress upon quantitative, or rather enumerative, ideas. We should distinguish between subscience, the mere study of objects, and science, the study of causes. Measurement is the very basis of science; and the neglect of

it and of logical deductions founded upon it are too common in biology. It is scarcely true to say that malarial fever is caused by a parasite and propagated by a mosquito: it can be caused only by many parasites and widely propagated only by many mosquitos. The *how much* and the *how many* are at least as important as the bare facts. I am convinced that if more exact results are desired, pathology must in the future tend more toward exact enumerative methods. As for epidemiology, it is principally a mathematical subject—the route of infection being given, the rate of infection depends upon laws like those which govern the diffusion of gases or heat; and ignorance of this has led to many wild statements regarding the spread and prevention of malaria. But the subject has yet to be developed, and Chapter V can approach it only in an elementary manner. Chapter VI is abbreviated owing to the details contained in the following one, to which it is largely introductory.

The contributions in Chapter VII are arranged geographically, as far as possible, and have been printed in the authors' words without modification. A useful index has been found impracticable; but a detailed table of contents is given at the end of the volume.

The omission of several important matters must be explained and apologised for. Some good campaigns have been left undescribed because I could not find reporters for them and failed in obtaining otherwise all the necessary particulars regarding them; not in any way because they should not have been included. A chapter on the local distribution and cost of malaria

would have been useful, and was attempted. The older books contain much on the former point; but the information given was based upon evidence which would scarcely satisfy us to-day, and accurate details could not be obtained without such long enquiry that I was forced to abandon the project entirely. Lastly, references to much good literature have been omitted or curtailed—not because such literature is of secondary importance, but because the object of the book is to present a general discussion of particular points rather than a complete record of all the known facts and hypotheses. But there is another reason for these omissions. Owing to grants recently given by the Advisory Committee for the Tropical Diseases' Research Fund (collected by the British Colonial Office), we are now preparing in Liverpool, not only for new researches, but for an exhaustive descriptive bibliography of malaria, in which, I trust, all the literature both of general and of local interest will be collated and compared. Indeed before long the whole of this immense subject will perhaps be dealt with in the only adequate manner possible—that is, by a special Bureau appointed for the study of it; and this book is intended to be a necessary preliminary to that work.

I hope that most of the matter contained in it will be of use to any one who proposes to undertake a genuine campaign against malaria. For this purpose we require a knowledge, not only of biological technics and of the various possible preventive measures, but also of the broad principles of sanitary statesmanship. The prevention of malaria on a large scale is a great economical

as well as a great humanitarian undertaking. A genuine campaign does not consist merely in the formation of inexpert committees, the passing of ordinances for the screening of water-butts, and the issue of wise advice to the public. To be permanently and economically successful, it must always be a permanent concern of the State, requiring careful measurements of the amount of sickness present, a nice appreciation of the measures most suitable for the locality, exact estimates of their cost compared with the cost of the disease, a well-considered organisation, and, above all, a fixed determination to succeed.

The world requires at least ten years to understand a new idea, however important or simple it may be. The mosquito theorem of malaria was at first ridiculed, and its application to the saving of human life treated with neglect, jealousy and opposition. But now, owing to the labours of many of those who have contributed to this volume, and of all of those who have so long and so patiently studied the subject, we are assured of final success. To them and to the memory of them this book is dedicated; but we must not rest content until the principles so well established by them are followed in every civilised country of the world. I hope, too, that mosquito reduction will before long be undertaken in most of the towns in the tropics, whether they are malarious or not.

RONALD ROSS.

University of Liverpool
1st June 1910.

P.S.—To the second edition of this work I have added in section 65 some notes on malaria prevention in India, and on some historical points; and also, at the end of the book, a more complete study of the mathematical part of the subject.

R. ROSS.

1st June 1911.

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THE PREVENTION OF MALARIA

CHAPTER I

HISTORY

1. Ancient Times.—Dr Richard Caton has kindly tried to ascertain for me whether there are any distinct references to malaria in the ancient Egyptian writings; but informs me that he knows of none in the medical papyri or temple inscriptions, and that Dr J. H. Walker confirms this statement. It should be noted that Strabo (first century B.C.) observed that Alexandria, in spite of marshes in the neighbourhood, was free from malaria in his time; and even at the present day the disease does not abound in Egypt as much as might be expected.

In Greece and Italy, however, malaria has been well known for more than two thousand years; and Greek and Italian authors have collected many references to it in old writings. Recently Mr W. H. S. Jones has given us a laborious and exhaustive study of the subject made in the light of our present knowledge [1907, 1909].

The first-named authors have always taught that the disease was very prevalent in those countries from the first; but Mr Jones, following a tentative suggestion of mine, gives many reasons in favour of the view that it may have entered them from without during historical times, and may subsequently have exerted considerable influence upon their civilisation. Thus, there are few references to it in the earliest literature,

but many in later writings. Homer (say 1100 B.C.) mentions what might be malaria, but only once (*Iliad*, xxi. 31); and Hesiod (say 735 B.C.), though he lived at Orchomenos on the shore of Lake Kopais, now intensely malarious,[1] and dealt with rural subjects, makes no clear reference to it. Theognis (say 550 B.C.) probably mentions it, and Heroditus (494 B.C.) possibly so. Jones infers from the medical writings attributed to Hippocrates that paludism was known in the medical schools before his birth (about 460 B.C.); and from the Wasps of Aristophanes (425 B.C.) that it "was attracting particular attention at Athens" at that time. About 429-400 B.C. the Athenians erected on the Acropolis a statue to Athena Hygieia, and introduced the worship of Asclepius, tending to show that "ill health was distinctly on the increase"; and after this "references to fevers became much more numerous," and malaria probably very common in Greece. He describes accurately and fully the effect which this must have had on its civilisation.

Apart from the literature, I am strongly of the opinion, based upon many considerations, that malaria could scarcely have been very rife in Greece before the height of its prosperity. The people were too vigorous and warlike. The gymnastic training of youths could scarcely have been possible if enlargement of the spleen had been very prevalent. The figures on the tombstones, though evidently idealised, do not suggest to my own medical apprehension anything resembling a malarious race; and the open-air life and ceremonies do not suggest a malarious country. The ancient flourishing population round Lake Kopais, for example, would have been simply impossible if the disease had been as rife then as it is now. It is now very rife there and in most of the Grecian villages; and there must have been a time when the change took place. Quite possibly it was introduced about the fifth century before

[1] In 1906 I found that twenty out of forty children at Orchomenos were suffering from enlarged spleen in June, that is before the malaria season.

Christ by soldiers or slaves from abroad, as it was certainly introduced into Mauritius in 1866 (section 30 (21)). Spreading gradually up the valleys, it would have tended to destroy rural prosperity, to drive the people into the healthier towns (as in Mauritius), and, assisted by other causes, to sap the vigour and physique of the race [Ross, 1906].

It is well known that the ancient Greeks recognised the quartan, tertian, quotidian and semitertian (probably malignant tertian) varieties of paludism, and many of its accidents; and were acquainted with its seasonal and local variability, and, above all, with its frequent prevalence near marshes. This last point is most germane to our present part of the subject. Thus there is an old story that Empedocles of Agrigentum (say 550 B.C.) "delivered Selinus (in Sicily) from a plague by draining its marshes, or by turning two rivers into them"—that he was able, as Matthew Arnold says, to

> "Cleanse to sweet airs the breath of poisonous streams."

Doubt has of course been thrown on the story; but whether it is true or not matters little. The mere fact that such a tale was told proves that the Greeks, even at an early date, had become in some way aware that marshes tend to generate sickness, and, still more important, that by drainage or other treatment this may be prevented. There are many passages connecting malaria with marshes. Thus as already stated Strabo (first century B.C.) says that Alexandria, in spite of its site, was free from marsh-fever even in his time. "It is to be inferred from this," Mr Jones says, "that damp places were generally known to be unhealthy, so that exceptions to the rule were noticed by observers as remarkable phenomena." Hippocrates (Airs, Waters, Places) noticed that those who live in low, meadowy and hot districts tend to be neither tall or well-built, but stout, fleshy, dark-haired, dark-coloured, bilious, and wanting in courage and endurance.

For ancient Italy we have a similar theory, namely, that

references to paludism are scanty in the earlier writers, but very abundant in the later ones. Mr Jones notices, both for Greece and Italy, that many of the most ancient settlements appear to have been made on sites which are now pestilential —suggesting that they were not so unhealthy when they were first selected. Professor R. C. Bosanquet tells me that some of the oldest settlements in Crete, made during the wonderful ancient civilisation of that country thousands of years before Christ, were situated at spots which are now intensely malarious. Many areas round Rome, now scarcely habitable, were the homes of great and prosperous peoples in the prehistoric period, and were later full of the country villas of rich Romans.[1]

Those who consider that malaria was always very prevalent in ancient Greece and Rome cite legends such as that of the destruction of the Lernean Hydra by Hercules—the Hydra being supposed to be symbolic of malaria. Lerna is a marshy district in Greece, and the Hydra was fabled to inhabit the marshes and to ravage the country round it. It is possible, however, that the fable refers merely to the drainage of the swamps for agricultural purposes. In Italy there was a vast and very old system of soil-drainage by *cuniculi*, probably constructed by the Etruscans ; and some have suggested that this drainage was carried out against malaria, and that it enabled the ancients to build villas at spots now deadly. Here, again, the object of the drainage was more probably agricultural. I should like to believe that it was a sanitary drainage, but find difficulty in doing so. Drainage against malaria must obviously be an urban and not a rural measure. The cost of draining all the country round Rome merely for sanitary purposes would have depleted the sanitary budget even of the logical ancients. Mr Jones's view appears the more probable — namely that the disease was possibly introduced at the time of the first foreign expansions, both in Greece and Italy, and that it gradually became intensified owing to the

[1] See also F. Genovese [1909].

causes described in sections 30 (20). North [1896] ascribes the intensification to rural depopulation—due to wars or to economical changes. I think it more probable that the malaria produced, or helped to produce, the rural depopulation; and we now have the living picture of the process before our eyes in Mauritius. Possibly also the disease has always had a tendency to eliminate or repel the fair strain of blood from the north, leaving the darker southern strains predominant. But the effect in Italy was probably less than in Greece, owing to the much smaller proportion of malarious area in the former.

Of course the Roman writers, both medical and non-medical, were acquainted with the leading facts about paludism mentioned above. The erudite Varro (116-28 B.C.) says in his *Rerum Rusticarum*, "Animadvertendum etiam si quo erunt loca palustria, et propter easdem causas, et quod crescunt animalia quaedam minuta, quae non possunt oculi consequi, et per aera intus in corpus, per os ac nares perveniunt atque difficiles efficiunt morbos"—that in marshes there are animals too small to be seen, but which enter the mouth and nostrils and cause troublesome diseases. Other famous passages are from Columella (about the first century B.C.), who says that bogs breed insects armed with stings, and pestilent swimming and creeping things, from which come obscure diseases. Here we have malaria connected not only with the marsh, but with insects or germs bred in the marsh. Cicero and Seneca say that paludism depopulated certain districts. Mr Robert Gladstone and Mr Jones have kindly called my attention to several passages referring to *mosquito nets*, called *conopeum* by the Romans, after the Greeks (our word canopy). Herodotus first noted with surprise the use of them in Egypt; and they are referred to later in Varro (*De Re Rustica* 2, 10, 8), Horace (*Epodes*, 9, 16), Propertius (3, 11, 45), Juvenal (6, 80), and Paulus Silentiarius (*Anthologia Palatina*). Horace says, "And among the military standards, oh, shame! the sun sees a

mosquito curtain ;" and Propertius calls these nets *foeda* (foul or disgraceful). Evidently the ancients felt towards them as do many of our own more manly colonists who prefer annoyance and even sickness to disgrace! But Paulus Silentiarius thought that they were useful for a post-prandial siesta in order to save the slaves the trouble of using a fly-flapper. Varro said that women, lately confined, spent a number of days in them; and Juvenal said that they were used to cover the cradles of the rich and noble.

On the 6th February 1905, Sir Henry Blake, Governor of Ceylon, called the attention of the Ceylon Branch of the Royal Asiatic Society to the fact that certain ancient Sinhalese writers, who lived certainly more than 1400 years ago, had connected fever with mosquitos. The original authority appears to be Susruta, in the chapter on Insects which forms the last chapter of his book on Poisons. He says that there are five kinds of mosquitos, one of which "produces the same symptoms as deadly insects." J. Jolly, however, discussed the subject [1905], and thinks that Susruta was merely referring to the irritation caused by the bites. He attributed malaria, J. Jolly says, to derangement of the humours. Personally, so far as I can judge, I doubt whether these writers ever really connected malaria, even in imagination, with the insects.[1]

2. Early Modern Times.—Little was added to our knowledge during the next thousand years; but about 1640 the inestimable boon of *Cinchona bark* was introduced into Europe. The Countess d'El Cinchon, wife of the Viceroy of Peru, had been cured of fever by means of it in that country, where it had been discovered by the Indians near Loxa (?); and she was wise enough to send it home to Europe. The use of it, after many checks, gradually spread; and in 1820 Pelletier

[1] I can ascertain little about malaria in early days in America; but according to a suggestive paper by O. Effertz, the disease was probably introduced there from Europe, just as the converse happened with syphilis [1909].

and Caventou extracted the alkaloid quinine from it. But the discovery of this specific has not only proved to be a blessing for the treatment of untold millions of human beings, but also enabled Morton [1697] and Torti [1753] to separate the malarial fevers, which are cured by it, from those upon which it has no influence, and by this means to differentiate and study the symptoms of the former. Morton also recalled the old hypothesis of the marsh; and this was amplified by Lancisi, who repeated the views of Varro and Columella in greater detail in his book *De noxiis paludum effluviis.* He stated that fevers disappear after drainage, and attributed the poison either to inorganic or organic emanations from the marsh. He studied mosquitos, and even suggested inoculation by them as a possible means of infection—though he also thought that their larvae foul drinking water [1717].

It is now apparent that the world had been gradually becoming aware during centuries of the paludic nature of malarial fever, not by direct experiment or even by investigation, but by a kind of subconscious experience based on public observations. In Italy especially, where of all civilised countries the disease was most prevalent, this process was most apparent —so much so that, as North describes [1896], the peasantry can often tell at sight which localities are likely to be "malarious." More than this, by similar general observation, the good effect of assainment of marshes had become equally notorious there. Thus, as early as 1667, Doni wrote a work called *De Restituenda Salubritate Agri Romani;* and references to a succession of works carried out on this principle, which I now call the principle of Mosquito Reduction, are given by Celli [1901]. At the same time efforts were made by many observers, such as Morton, Lancisi, Lind, Pringle, to explain the paludic connection; and these resulted in the formation of the hypothesis of the *paludic miasma.* This was supposed to be some kind of infecting emanation from stagnant water, either chemical, or as Lancisi suggested, organic; but in no

cases, apparently, were experiments made to test the point. Later, when it was observed that malaria may sometimes occur where there is no marsh, the hypothesis of the paludic miasma was extended to that of the telluric miasma, according to which the poison exists not only in marshes but anywhere in suitable soil, from which it rises at night or when the soil is disturbed. This speculation, for it is nothing more, is not quite dead even yet, though the observation which originated it is easily explained otherwise. The word "malaria" (*mal' aria* or bad air) is derived from it.

3. Discovery of the Parasites. — Last century, however, pathological science was no longer content with mere guesses at the truth, but began to demand strict microscopical and experimental evidence. Applied to paludism, this method gave an early reward. In 1847, H. Meckel discovered innumerable black granules in the blood of an insane patient; and the discovery was subsequently confirmed and amplified by Dlauhy, Virchow, Heschl, Planer and Arnstein—the granules now being known under the name of the *paludic pigment*, or *melanin*, or *haematozoin* (Sambon).[1] For a long time they were thought to be due to a chemical action of the paludic miasma on the red cells of the blood.

About the same time many laborious attempts were made to discover some animal or vegetable organism which lives in marshes and produces paludism. As long ago as 1846 Rasori (cited by S. Calandruccio) made an extraordinary prediction on this subject. "For many years," he said, "I have held the opinion that the intermittent fevers are produced by parasites that cause the successive paroxysms of fever by their reproduction, which occurs periodically more or less rapidly according to their species"—and this has proved to be exactly true. Many writers attributed the disease to various marsh-growing vegetables. In 1862 Salisbury in particular, after considerable

[1] I suggest the name Plasmodin, which is still more exact.

study, blamed a kind of *Palmella;* and after 1878, a number of Italian workers, Lanzi and Terrigi, Edwin Klebs and C. Tommasi-Crudeli, thought that they had actually incriminated certain fungi or bacteria, which they said swarm in malarious places, occur in the blood, produce spores before each paroxysm of fever, and cause similar infection in animals. These findings were even confirmed by Marchiafava and other Italians; but have now been completely discredited.

In 1878, however, A. Laveran commenced his studies of the subject at Bône in Algeria, by following up the granules of pigment, already referred to, in the blood of living patients. He was struck by the fact that they were frequently contained within cells possessing active amoeboid movements ; and, finally, on the 6th November 1880, at Constantine, he detected the microgametes issuing from the male cell. Though at the time he did not know the nature of this phenomenon, it convinced him that he was dealing with a living parasite of the human red corpuscles. In fact the black granules are merely the excrementitious matter produced by the parasites from the substance of the red cells, and contained within their bodies, or released in the tissues of the host.[1]

About the same time C. Gerhardt proved that healthy persons can be infected by the inoculation of blood of patients suffering from paludism [1884]. His experiments were afterwards verified by many workers, and demonstrate (apart from the microscopical discovery of the parasites) that the disease is not due to any gaseous emanation from marshes, but is a true infection by some living virus.

In 1886 and subsequently, C. Golgi, who was favourably situated in Pavia for the work, showed clearly that the parasites

[1] Recently R. Blanchard (*Archi. de Parasitologie*, vol. vii. 1903) would have us believe that one P. F. H. Klencke had discovered the parasite of malaria before Laveran ; but it is perfectly obvious from Klencke's drawings, given by Blanchard, that his "parasites" are merely the usual artifacts found in fresh blood. Klencke, moreover, was not studying malaria at all, but "vertigo." Medical literature is full of such simulacra.

reproduce by simultaneous sporulation; that the febrile paroxysm in the patient commences at the moment when these spores are liberated (just as Rasori had divined); and that the parasites of quartan and mild tertian fever are morphologically different [1886]. A little later, Canalis, and Marchiafava and Celli discovered similar facts regarding the malignant parasites, and showed that they differ from the quartan and mild tertian parasites; and Marchiafava and Bignami suggested that they are of two varieties, the malignant tertian and the quotidian. In 1885 Danilewsky discovered similar parasites in birds and several other animals; and subsequently Marchiafava, Celli, Bignami, Mannaberg and others, made many careful studies of the parasites, and of their effects in human beings; Romanowsky found the proper way to stain them; and many observers verified these researches in various parts of the world—the literature amounting to some hundreds of publications.

4. Speculations regarding the Mode of Infection.—But the question now arose—an all-important question in connection with the prevention of the disease—How do these parasites manage to effect an entry into the blood of men and animals? Most observers, remembering that the disease often abounds in the neighbourhood of marshes, assumed at once that Laveran's parasites must be capable of living in a changed form in stagnant water; and some actually sought them there. Thus Grassi and Calandruccio suggested that a free living amoeba is really the external stage of the organism. On the other hand, experiments to infect healthy persons by water from marshes, made by Marchiafava and Celli [1885], Marino [1890], and especially Agenore Zeri [1890] failed entirely. Zeri gave marsh water to nine persons to drink in doses from 1·5 to 3 litres a day for a number of days. To another five persons he gave the water by clyster; and to sixteen by spray inhalation—yet no paludism followed. Like all negative results, his are not absolutely conclusive—since the failure may have been due to some unforeseen

condition absent from the experiments; but his paper is too important to have been forgotten as much as it has been.

Before describing my own Indian researches which commenced at this point, it is necessary to survey our general parasitological knowledge, together with the state of thought regarding malaria, at the time. Discoveries concerning Laveran's bodies, which are animal and not vegetable organisms, have a line of descent from previous discoveries concerning animal parasitology, and not directly from bacteriology.

Many of the large animal parasites of man and animals have been known to us from antiquity, but until the beginning of the eighteenth century, were supposed, like other low forms of life, to be created in each host by "spontaneous generation." Even long after Redi (1668) proved that this hypothesis did not hold for certain insects, it was still supposed to apply to the parasites. Gradually, however, large numbers of the latter were discovered, and Pallas put forward the view that they originate, like other animals, from eggs, which escape from an infected person or animals, and are by chance swallowed by another host. Such a history is, in fact, quite correct for many parasites; but in 1790 Abildgaard made the remarkable discovery, by experiment, that the parasites, called *Bothriocephalus solidus* and *Ligula*, inhabit for a part of their lives certain fish, and reach maturity only in certain water-fowl which happen to swallow the infected fish. This appears to have been the first found instance of the wonderful process of metaxeny,[1] or change of host. The idea was lost sight of for years until Eschricht in 1841, and Steenstrup in his famous work on the alternation of generations of Trematodes (1842), recalled it. It was finally established for Cestodes by F. Küchenmeister, physician to the Duke of Saxe Meiningen, in 1851-1853, by means of direct experimental feeding methods first used by him. Metaxenous

[1] I use this word on the authority of De Bary, who employs the incorrect form *metoxenous* in the above sense—see his book on Fungi, Mycetozoa, and Bacteria. Trans. Clarendon Press, Oxford, 1887, p. 387.

parasites are those which spend part of their life in one kind of animal, and the rest of it in another—generally the early part of their existence in one host, and the mature sexual part of it in another host which preys upon the first one, as in the rabbit and the dog, the mouse and the cat, swine and men, and so on. Moreover, he proved that the eggs or young of the mature parasites are able to pass back again into individuals of the first species of host, from which they are again transferred to the second species of host—and so on, *ad infinitum.*

This great discovery, which may almost be called the basis of modern parasitology, was rapidly verified by Küchenmeister and Leuckart for many parasites. At first it was applied to those of the higher animals; but before 1858 Leuckart discovered that the Nematode worm, *Cucullanus elegans* of the perch develops in the little aquatic Arthropod called *Cyclops*, or the Water Flea. Next, perceiving the resemblance of this worm to the famous *Filaria medinensis*, or Guinea Worm of man, he suggested in the same year (1858) to the young Russian naturalist and traveller, Fedschenko, that the latter parasite may also pass a stage of its life in a *Cyclops.* The Guinea Worm lives under the human skin, and emits its young through an ulcer which it produces in the integument—the young next finding their way into water. A few months later Fedschenko proved by experiments in Turkestan that Leuckart was right. He watched the development of the embryos in the *Cyclops;* but did not complete the cycle by infecting men from the latter. He supposed, however, that men become infected by accidentally swallowing infected *Cyclops* in drinking water.[1]

[1] For an account of this discovery see R. Leuckart, *Die Menschlichen Parasiten*, 1876, vol. ii. p. 704. As it is an extremely important discovery, I have been at considerable pains to ascertain the exact dates. It will scarcely be believed, but Leuckart's complete works are difficult to obtain in this country. I am indebted to Dr R. T. Leiper for having studied the question. The facts appear to be as follows: In 1858 Leukart (when at Naples) suggested the idea to Fedschenko, who was then about to proceed to the East; but did so as a secret (?). In 1865 and 1866 Leuckart first published his account of the development of *Cucullanus* in *Cyclops*, but merely hints at the idea that *F. medinensis* has a similar development, without mentioning *Cyclops* as the possible second host. In 1869 Fedschenko seems to have returned from

In 1868 another important case was discovered by Leukart and Melnikoff in Leuckart's laboratory. They found that the young of *Dipylidium caninium* (*Taenia cucumerina*), a parasite of dogs and cats, develops in the dog-louse[1]—this being, I believe, the first case in which a parasite of a mammal was found to be transferable by any kind of vermin which feeds on that mammal. The great credit due to Leuckart in connection with these discoveries has been much overlooked of late.

Filaria bancrofti, a human parasite, was discovered in the adult form by Bancroft in 1876, though its embryos had been found by Demarquay in 1863 in chylous fluid, and by Lewis in blood in 1872. In the *Lancet*, 12th January 1878, Cobbold mentions a suggestion of Bancroft, dated the 20th April 1877, that the worm (which is closely allied to *F. medinensis*, and indeed to *Cucullanus*) might be carried by mosquitos. In 1877, P. Manson, in Amoy, China, came to the same conclusion, and on examining a number of mosquitos fed on a Chinaman whose blood contained the embryos, discovered that these develop in the tissues of the insects just as the embryos of *F. medinensis* develop in *Cyclops*.[2] This was the first time that metaxeny was found to occur with any parasite between man and mosquitos; but Manson did not observe the development of the embryos to a stage much more advanced than that observed by Fedschenko in the *Cyclops*. He thought also that mosquitos die on the surface of water a few days after feeding on blood; and that the partially developed embryos then liberate themselves from the dead insects, and, after swimming free in the water, are swallowed by human beings. But he did not prove this; and indeed, in 1900, G. C. Low and S. P. James, acting on the evident suggestions of my work on

Turkestan and to have published his paper in Russian, describing the development of *F. medinensis* in *Cyclops*. The last, then, must be taken as the date of publication of the discovery. Dr A. Jacobi, Leuckart's biographer, can give me no further information.

[1] R. Leuckart, *Die Menschlichen Parasiten*, 1876, vol. ii. p. 704.

[2] P. Manson, *Filaria sanguinis hominis and Certain New Forms of Parasitic Diseases*. London, 1883.

malaria, made observations which lead us to think that the embryos return to human beings through the insect's proboscis. By his detailed studies, however, Manson has built up most of our present knowledge on the subject of filarial disease in man.

It should be noted here that all these examples of metaxeny apply only to the higher parasites, the Helminths or Worms, and that in some of the cases, including those of Fedschenko and Manson, the life-cycle had been by no means completely ascertained — so much so that many doubted whether the observed development of the embryos in *Cyclops* and *Culex* could be looked upon as a genuine stage of metaxeny. But the bodies discovered by Laveran belong to a much lower, in fact to the lowest, class of animals—the Unicellular ones. Though many organisms of this class—amoebas, gregarines, coccids, trypanosomes—have been discovered in various animals, yet up to 1889 not one of them had been proved to be metaxenous. During this year, however, a remarkable discovery was made by Theobald Smith and F. L. Kilborne in America in connection with the disease called Texas cattle-fever. For a long time the farmers had thought that it is caused by certain cattle-ticks. Smith and Kilborne now showed that the disease is due to minute parasites of the red corpuscles, akin to the parasites of malaria but not belonging to the same group, called *Piroplasma bigeminum;* and that they are inoculated in some way into healthy cattle by the bites of young ticks born from a parent tick which has fed upon a diseased ox. This fact was conclusively proved by direct experiment, and suggested that these *Piroplasmae*, which are unicellular animals, might also be metaxenous like so many of the higher parasites. But the observers failed in following out microscopically the life-history of the *Piroplasma* in the ticks, and therefore did not actually prove any alternate generation in them.[1]

[1] T. Smith and F. L. Kilborne, *Investigations into the Nature, Causation and Prevention of Texas or Southern Cattle Fever.* Government Printing Office, Washington, 1893.

Meantime, however, there had been many vague conjectures to the effect that several diseases may be produced by biting vermin. Thus, as just mentioned, American farmers had long thought that Texas cattle-fever is carried by cattle-ticks. Many travellers also related that African natives ascribed a peculiar sickness to the bites of another kind of tick ; and others said that the deadly nagana of cattle in parts of Africa is probably due to the bites of the tsetse fly. And similar speculations connecting both yellow fever and malaria with mosquitos had long been rife. Generally derided at the time, these views were speedily forgotten ; and it is only of late that many of them have been resuscitated and discussed as instructive relics of the past. It will, I think, interest the reader to follow the gradual development of our knowledge of this important subject.

I have already mentioned the curious utterances of Varro, Columella, the ancient Sinhalese books, and of Lancisi, regarding insects and fever. Nuttall [1899, p. 75] gives statements by Lustig, Rubner, Koch and myself, to the effect that the peasantry in Italy, Tyrol, East Africa, and Assam seemed to have vague ideas of the same kind. Dr R. H. Kennan informs me that he has found an old ordinance of Freetown, Sierra Leone, dated 1812, in which the inhabitants (mostly freed slaves) are enjoined to keep the road in front of their plots in good condition in order to prevent the formation of "stagnant pools which generate disease and mosquitos over the town." In 1848, Dr Josiah Nott, of Mobile, Alabama, appears to have stated that both yellow fever and malaria may be transmitted by mosquitos, and refers to the speculation as having been already advanced as regards malaria. In 1854, however, Louis-Daniel Beauperthuy, a French medical man, born in Guadeloupe in 1808, gave the hypothesis in greater detail [1854]. As a "travelling naturalist" for the Paris Museum in Venezuela, he studied both these diseases microscopically, and concluded that they are produced by a venomous fluid injected under the skin by mosquitos, like the poison injected by snakes. Marshes, he

says, are dangerous because of the mosquitos bred in them, not because of their effluvia. He stated that several species of mosquito carry yellow fever, but mentioned especially "the *zancudo bobo* with legs striped with white" — probably the *Stegomyia calopus*,[1] which we now know is really the agent. But he mentions no experiments in support of his opinions, which seem to be only ingenious speculations based upon general thought and observation.

In 1881, and subsequently, Charles Finlay of Havana repeated a similar hypothesis, apparently independently of Nott and Beauperthuy [1881]. His views, however, differ in an important particular. While Beauperthuy seemed to think that mosquitos originally obtain the disease-giving poison from the marsh in which they breed (and Varro, Columella, and Lancisi believed the same), Finlay held (regarding yellow fever) that they obtain it from sick people. In other words, he thought that the insects simply convey it from patients to healthy persons. The proboscis of a mosquito which bites a patient becomes contaminated by germs in his blood; the germs multiply in the proboscis; and then enter the blood of any person whom the mosquito bites next—just as bacteria may be carried on a soiled surgical instrument from one person to another. He thought that an insect which had only just bitten a patient could convey the virus; but that the longer it lived after biting the patient the more the germs would multiply in its proboscis, and the larger the dose given to the healthy person would be. He also considered that the mosquito with striped legs (*Stegomyia calopus*) is the agent of yellow fever. He records some experiments; but they must have been very doubtful, since we now know that mosquitos which have bitten a patient must live for no less than twelve days before they can infect a healthy person. Like Beauperthuy, he rightly conjectured the species of mosquito which carries yellow fever, and actually placed them in the hands of the men who ultimately solved the problem.

[1] J. Guiteras says *not* (Lancet 18th June 1910).

He was acquainted with Manson's researches on the development of *Filaria bancrofti* in mosquitos.[1]

In 1883 Dr A. F. A. King wrote an able paper on the subject [1883] in which he gives no less than nineteen reasons why mosquitos are likely to carry paludism. These are:—(1) that both paludism and mosquitos are connected with marshes; (2) that they both require a temperature of over 60 degrees F.; (3) are checked at freezing point; (4) abound most near the equator and sea-coasts; (5) have an affinity for dense foliage; (6) can be screened off by trees; (7) can be transported by winds; (8) are encouraged by turning the soil; (9) are affected by "bodies of water"; (10) are diminished by cultivation and settlement; (11) keep near the surface of the ground; (12) abound most after sundown; (13) and in the open; (14) are destroyed by fires; (15) are not so common in cities; (16) are most prevalent in autumn; (17) are arrested by mosquito nets; (18) affect infants (which are generally protected by nets) less than adults; and (19) attack whites more than other races. This was by far the best exposition yet given. Though arguments (4), (13), (14), (18), and (19) are not sound, while arguments (5), (6), (7) are very doubtful, and though the most cogent argument of all was not known to him, the cumulate effect of his careful and well-arranged reasoning was very strong. Like Beauperthuy, he held that the insects bring the poison from the marsh and inoculate it by their bites. He was acquainted with Manson's work, but not with Laveran's. His paper was speedily lost sight of, and was not resuscitated until my researches had cleared up the question.

About the same time Laveran suggested the same idea [1884, p. 457] in a short sentence: "Do mosquitos play the same rôle in paludism as in filariasis?" he said. "The thing is not impossible, and we must note that mosquitos abound in all marshy places."

[1] For further details see the interesting book, *Mosquito or Man*, by my colleague, Sir Rubert Boyce. John Murray, London, 1909.

Also about the same time, during his famous studies on cholera in India, R. Koch had the same notion; but he mentioned it only in his lectures to students [Nuttall, 1899, p. 77, and Ross 1905, p. 73].

Ten years later, and some years after I had commenced my studies of the subject,[1] P. Manson supported the mosquito hypothesis in a rather short paper [1894]. But he added a new and a stronger argument to those already given. In the blood of man the parasites of malaria consist chiefly of forms which reproduce themselves indefinitely by spore formation; but in addition to these there are other forms which appear to possess no function in the human body, but which, shortly after the blood containing them is drawn from the patient's finger, often emit long and actively motile filaments which may break away from the parent cell, and wriggle about rapidly in the fluid under the microscope. For a long time these bodies had been the subjects of discussion. Some observers, chiefly of the Italian school, held that they represent only the dying struggles of the parasite; others, including Laveran and Danilewsky, thought that they are really living bodies; and Mannaberg even suggested that they may be connected with the life-history of the parasites outside the human body—though he did not explain how this could be. Manson now offered an explanation. He thought that when a mosquito sucks the blood, these parasites enter its stomach with the blood, and there, in a few minutes, emit their motile filaments just as they do under the microscope. The motile filaments, he thought, were flagellated spores, which next pass through the walls of the mosquito's stomach and take up their abode in its tissues, where they must develop further. Two years later he repeated this hypothesis [1896] with the assistance of my preliminary researches, but added some conjectures as to the future fate of the "flagellated spores." He still thought that mosquitos die on the surface of water a few days after laying their eggs in it. The malaria germs

[1] Of malaria, not of the mosquito hypothesis.

might now escape from the dead insect into the water, with which they might be swallowed by men; or they might be blown about with the dust and be inhaled; and they might also be swallowed by mosquito larvae and so propagate themselves indefinitely in the insects, apart from man. Roughly, the life-history of Laveran's bodies was he thought, similar to that which he considered *Filaria bancrofti* to possess.

A few months later A. Bignami attacked Manson's views for several reasons [1896]. He refused to admit that mosquitos can take the parasite from human beings, but supposed, conversely, that human beings take it from mosquitos. In support of this he merely adduced some of the reasons previously given by King. Several other writers supported or opposed the hypothesis about the same time.

This is a short, but, I think, a fairly exact account of these hypotheses. The importance of speculations of this kind is apt to be either underrated or overrated. Modern science does not look upon them with much favour unless those who publish them strive also to verify them. It is easy to sit at home and weave many hypotheses; but attempts to verify them generally demand endless labour, expense, self-sacrifice or even danger, and often fail, or, even if successful, win little reward. There were many before Columbus who imagined America; but between the dream and the reality an ocean had to be traversed. All the conjectures mentioned above have proved wrong in many particulars. Mosquitos do not bring the virus from the marsh to the man, nor from the man to the marsh. The truth has proved to be far more wonderful than any hypothesis. It is curious that no one recognised the suggestion which should have been obtained from the phenomenon of metaxeny among some other parasites, namely, that the mosquitos might carry the virus from man to man. I was driven to this in 1896-1897 just before the fact was revealed.

On the other hand, such speculations often serve a useful purpose by giving some direction to practical work. Thus

those connected with Texas cattle-fever, nagana, and tick-fever greatly limited the labour of research by suggesting the actual species of the alternative hosts, namely certain ticks and tsetse flies; and those of Beauperthuy and Finlay suggested the *Stegomyia calopus* as the agent of yellow fever. With regard to paludism the earlier hypotheses, critically looked at, suggested nothing more than that there might possibly, or probably, be some connection between mosquitos and the disease. Manson's idea regarding those forms of the parasites (gametocytes) which produce the motile filaments (microgametes) was, however, more than a mere speculation—it was an induction based upon our general knowledge of parasites. As he said, these bodies must have some meaning and object. It was difficult to imagine that they could have any other object except to infect some suctorial animal—probably mosquitos. But this idea led us no further; it gave no clue as to how and where the parasites live in mosquitos, how they return to man and infect him, and in which of the hundreds of species of mosquitos they exist. It was only a glimmer in the darkness—but it was something. Obviously the truth could be obtained only by a long and determined investigation of the whole subject.

5. Researches regarding the Mode of Infection.—These really began with the old attempts mentioned in section 3 to find the infective organism in marsh water, and with those mentioned in section 4, and culminating in the negative efforts of Zeri [1890] to infect healthy persons with such water. Parallel researches on various worms and on *Piroplasma bigeminum* were referred to in the last section. I have now to describe my own work. Entering the Indian Medical Service in 1881 I was much struck by the misery caused among the people by this and other diseases; and in 1889, during leave in England, studied bacteriology and public health, with a view to undertaking pathological investigations. On returning to India I was especially drawn to the difficult problem of

paludism, regarding which the facts observed by me did not accord well with the old hypothesis of a marsh miasma. I was at first led to the ideas of Broussais that the disease might be due to intoxication from intestinal organisms. In those days it was almost impossible to obtain much literature in India; and Laveran's discovery was obscured by writers who, in attempting to find his parasites in the blood, found only natural objects which resembled them. I detected the error and, with many others, doubted his discovery in consequence, and failed to find his organism—though I obtained much valuable practice in microscopical work. In 1894 I returned to England, where P. Manson showed me the true Laveran's bodies. He also told me his new mosquito hypothesis; and I reminded him that the same idea had been mooted by Laveran. Next year I returned again to India, determined to work out the whole subject thoroughly. The details have been previously recorded in my Nobel Lecture [1905], but the reader should know the following points.

Little was then recorded regarding the structure, habits, or classification of mosquitos; and I could obtain no literature on the subject, and was obliged to find out everything, including the technics, for myself. I was not then aware of the speculations of Beauperthuy, Finlay and King, or the discovery of Smith and Kilborne.

There was nothing to guide me as to the species of mosquito concerned, as we had no right to assume that the malaria-bearing species is necessarily the commonest in any malarious locality; and as metaxeny was not known at that time among unicellular parasites of animals, there was nothing to indicate the form or position that Laveran's bodies would, by supposition take in the insects. The study of these minute organisms was moreover much more difficult than that of the large worms already investigated; and the proper method of staining was not then known.

In May 1895 I fed *Culex* and *Stegomyia*, bred from the larva

on patients with the gametids (crescents) in their blood, and showed that the motile filaments were, as usual, extruded in the insects' stomachs. But I speedily found that, owing to their minute and delicate structure, it was impossible to follow them further, as Manson had suggested should be done. I was therefore obliged to adopt a new procedure of my own. The insects were kept alive after being fed for several days, and were then exhaustively searched for any parasites they might contain. If such parasites were found it was possible that they might be the developed motile filaments; and the point could be cleared up by subsequent experiments. This method was extremely laborious, but was the only one possible. For more than two years the results were completely negative.

Meantime, following the later conjectures of Manson, I tried to infect healthy persons by giving them to drink water in which fed mosquitos had died. By some accident the first case appeared successful; but twenty-one succeeding experiments practically failed [1896].

For this and other reasons I abandoned this part of Manson's hypothesis, and began to think that the insects probably carry the parasites from man to man, either depositing them by defaecation on the skin of healthy persons, or inoculating them under the skin after puncture. In August 1896, Mr Appia, Assistant Surgeon of the Civil Hospital at Bangalore, kindly submitted to the suggested experiments. Many *Stegomyia* and *Culex*, previously fed on patients, were fed on him a few days later. There was no result—the mosquitos being of the wrong species, and the period between the feedings being too short [1896].

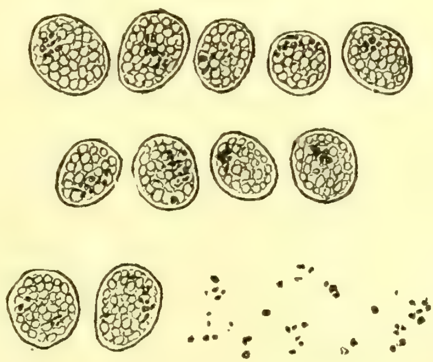
Bignami now criticised Manson's hypothesis [1896]; but I made a number of experiments in support of it, and showed that the extrusion of the motile filaments is really a living process and not a "death agony" as many had thought [1897].

My repeated failures, however, now persuaded me that I had probably been working with the wrong kinds of mosquitos—

Culex and Stegomyia; and in April 1897 I went to an intensely malarious spot near Ootacamund in the Nilgiri mountains, in order to see if I could find another likely kind. There, for the first time, I saw an Anopheline—the really culpable kind; and also made several observations which made me finally abandon the second part of Manson's hypothesis and adopt the view that the insects carry the parasites in some way from man to man [1898].

A few months later I returned to Secunderabad, where I had commenced my mosquito work in 1895, and was at last rewarded with success. On the 20th and 21st August I found the zygotes of the parasites in two large dapple-winged mosquitos which had been bred from the larva, and fed on a case containing crescents. This fortunate observation gave the clue to all that followed, because it indicated the form and position of the parasite in the insect, and also the variety of insect capable of carrying it. The problem was practically solved, and only details and formal proofs required to be ascertained.

FIGURE I.



The matter was reported at once to Government, and the medical press [1897]. Unfortunately no more of the large dapple-winged mosquitos could be obtained, but I found the pigmented cells in a *Culex* which had been caught feeding on a case of tertian malaria, and also in one of a smaller kind of dapple-winged mosquito. On this I wrote to Manson telling him to expect the full solution of the problem in a few weeks. Next day, however, I was ordered to proceed to Kherwara, a place a thousand miles distant, where there was little malaria at the time. Owing to this interruption I have never been able to identify exactly the two species of dapple-winged mosquitos; but from their general characters, the markings on their wings and the shape of their eggs, they were obviously Anophelines.

I remained at Kherwara from September 1897 to February 1898, when the Government was good enough to put me on special duty to continue my researches in Calcutta. On arrival there I found it impossible to work with human malaria, chiefly on account of the riots caused by Mr Haffkine's anti-plague inoculation, and therefore attacked the malaria of birds.

According to Manson's hypothesis, the motile filaments were flagellated spores capable of living in mosquitos' tissues; but my pigmented cells contained plasmodin, which cannot exist in spores. The discrepancy was explained by a series of researches made in 1896. Metchnikoff and Simond had found similar motile filaments in *Coccidium oviforme* of rabbits, and suggested that they are really *sperms* and not *spores* [1897]; and a little later MacCallum and Opie in America actually observed, in the case of one of the malaria parasites of birds (*Halteridium*) and one of those of men, these sperms in the act of fertilising the female cell [1897]. My pigmented cells were therefore the fertilised cells, or zygotes, still carrying the plasmodin which they originally possessed.

In March 1908, I found these bodies again in *Culex fatigans*

fed on birds containing *Plasmodium* (*Proteosoma*) *danilevskyi*. In a few months, by means of rigid experiments, the development of the parasites in the insects was fully ascertained. On the 4th July I observed the extraordinary fact that the protospores (sporozoites) of the parasites enter the insects' salivary or poison glands, thus suggesting that infection is produced by the bites of the insects. I had thought that this might be the case, but none of us had ever really imagined such a wonderful process —nature surpassed the divination of all of us! In July and August I infected twenty-two out of twenty-eight healthy sparrows and some other birds, by means of the bites of *Culex fatigans* which had been previously fed on infected birds.

These experiments gave the fundamental solution of the problem, and the first proof of metaxeny among unicellular parasites of animals—just as Küchenmeister's had proved the process to hold for higher organisma. They exhibited, with the assistance of MacCallum and Opie's observation, the whole development, step by step — the position and form of the parasites in mosquitos, and the wonderful method of infection. Owing to the great similarity of the avian and human parasites, there was no doubt that the latter had the same history—and indeed the stages of these up to the fifth day had already been found by me a year previously in Anophelines.

I was now anxious to complete the work on human malaria, but was ordered to leave the subject and attack the difficult one of kala-azar. This, with the writing of the necessary report, occupied me for nearly six months, until I left India in February 1899. I then joined the newly-formed Liverpool School of Tropical Medicine; and at the end of July set out with E. E. Austen, Entomologist at the British Museum, and Dr H. E. Annett, of our School, to complete my study of the human parasites. In a few weeks I was able to show that the parasites of quartan, tertian, and malignant fever all develop in *Pyretophorus costalis* or *Myzomyia funesta*, precisely as the *Proteosoma* of birds develops in *Culex fatigans*. This

completed the task; and I now turned to the subject of practical prevention.

6. Confirmations and Extensions.—My observations of the parasites in mosquitos were published by myself in a series of papers on 18th December 1897, 26th February 1898, 21st May 1898, 11th October 1898, and 24th January 1899; and also, owing to official delay in regard to some of my reports, by Manson on 18th June 1898, July, and 24th September 1898. My paper [May 1898] contained a full account with plates of the necessary technique invented by myself, and of the stages of *Proteosoma* in the mosquito up to the sporulation of the zygotes—the mode of infection being published in the later papers by Manson (to whom I had telegraphed the news in time for the Annual Meeting of the British Medical Association at the end of July), and also by myself [October 1898]. Moreover, I had sent numbers of my specimens to him and to Laveran; and, in fact, by August 1898 my researches were well known to most workers at the subject in Europe and America.

The first to confirm them was R. Koch, with Kossel, who in September followed completely the development of *Proteosoma* in *Culex nemorosus*, and demonstrated a point which I had not had time to deal with, namely, how the zygotes traverse the wall of the insect's stomach [1899].

In December, C. Daniels, sent by the Royal Society to examine my results, arrived in Calcutta and confirmed them [1899]. He and Dr Rivenberg also assisted me greatly at that time.

Until my researches were published, the Italian writers had generally disbelieved in the mosquito hypothesis, though A. Bignami had accepted it on the lines of Lancisi and King. Now, however, they made strenuous efforts to follow; but they reached no tangible result until November, when Bignami, in the light of my experiments on birds, infected a man in Rome with mosquitos brought from Maccarese [November 1898]. A

few weeks later A. Bignami, G. Bastianelli and B. Grassi found the zygotes of malignant malaria in *Anopheles maculipennis*, fifteen months after my original success with the same parasite in Secunderabad, and four months after the whole life-cycle of the parasites, as exemplified in *Proteosoma*, had been fully published by Manson and myself. They were thus able to forestall the completion of my work on the human parasites, but did so entirely by following my previous researches, while my time was being wasted over a lengthy report on kala-azar—a duty which I was not permitted to postpone. Being very favourably placed for the work, and not being subject as I was to interruptions, they succeeded in infecting three more persons, and were able to publish numerous papers with coloured pictures, which greatly popularised the subject. They proved one important new fact, however—that the tertian parasites also are carried by the Anophelines; and I verified this later in Sierra Leone.[1]

[1] The relations between the work of these authors and of myself are exactly discussed in my lecture [1905]. For a long time some writers attributed to them the discovery of the mosquito-cycle of the important human parasites, and to me only the study of the humble *Proteosoma* of birds. But according to zoological rules, priority in the discovery of the life-history of any group of organisms belongs to that investigation which first discloses the new life-history in any member of the group—which is only just, since there is usually little difficulty in repeating the same observations for other members of the group. The life-history of *Proteosoma* therefore holds the priority in the case of all similar life-histories; and is in fact identical with those of the human parasites, and indeed almost the same as those, since found, of other unicellular organisms. Efforts were also made, with the same object, to discredit my original successes of 1897 with the human malignant parasites by the statement, contrary to the printed evidence of my words [December 1897], that the mosquitos used by me had not been bred from the larvae—although the men who made these efforts were apparently actually committing at the time the very fault of which they wrongly accused me. In a later work B. Grassi occupied many pages in trying to persuade the world that he had discovered the "Anopheles malariferi indipendentemente da Ross" [1900, p. 31]. Unfortunately for him he had referred to my work in a paper [October 1898] which he had published two months before he "discovered" anything at all. There is no doubt that he and his colleagues recognised the genus of my "dapple-winged" mosquitos from several statements of mine, and were thus able easily to find the parasites in members of the same group in Italy. The Italian work was due mainly to Bignami and Bastianelli, and not to this author as erroneously stated in many monographs and articles. Laveran has made similar remarks about his own work [1907, p. 13]; and the matter is mentioned here because such misstatements are apt to discourage genuine work [Ross, 1905].

In November 1899, R. Koch, who has given such great discoveries to pathology, added another in connection with malaria. He found in the valley of Ambawara in Java that while large numbers of the native children showed the parasites in their blood, the adults seemed to be comparatively free from them, and had obviously become partially immune. Thus in most very malarious places it is chiefly the children who suffer from the acute disease. The blood of those who survive gradually produces something which after a number of years has the power of reducing and perhaps extinguishing the parasite invasion. From this it follows that in such localities the Anophelines must become infected principally from the native children (*see* section 31 (9)).

In the summer of 1900, P. Manson carried out an important crucial experiment. A number of *Anopheles maculipennis*, fed on cases of mild tertian, were brought from Italy to London, and were allowed to bite P. T. Manson there on several occasions. He developed the disease on the 13th September, the tertian parasites being found in his blood a little later. The insects were also allowed to bite G. Warren, who was similarly attacked fourteen days afterwards. At the same time L. Sambon, G. C. Low and two others lived for three of the most malarious months in one of the deadliest places in the Roman Campagna, Ostia, without contracting the disease, because they spent the nights in a hut protected by wire gauze against the entry of mosquitos.

On many other occasions healthy persons have been similarly infected on the lines of my experiments with birds in July to August 1898. As already mentioned Bignami and Bastianelli infected four persons in Rome (which is itself free from malaria) in 1898-1899. Subsequently C. F. Fearnside at Rajamundri in India infected seven out of eight persons, including himself, in 1900-1901 [1901]. W. Schüffner infected himself and two others at Delhi, Sumatra, in August and September 1901, two with tertian and one with malignant

malaria, by means of certain Anophelines [1902]. N. Jancsó infected ten out of fifteen persons at Kolozsvar in Hungary in 1904 by means of *A. maculipennis* (section 17).

My work on the *Proteosoma* of birds has been confirmed by Koch [1899], C. Daniels [1899], B. Grassi [1900], R. Ruge [1901], Ed. and Et. Sergent [1907], R. O. Neumann [1908].

The mosquito cycle of the human parasites has been further worked upon by Fernside [1901], Stephens and Christophers [1899-1903], Schüffner [1902], Jancsó [1904], Schaudinn and others.

Reviewing this history we shall see that the great stream of research on malaria, descending to us through more than two thousand years, is composed of three main tributaries finally mingled together. One tributary rises in the work of the ancients on the different clinical forms, and consists of the discovery of the cinchona bark; the work of Torti (1753); the discovery of the plasmodin by Meckel (1847); of the parasites by Laveran (1880); and of the confirmations and extensions of Golgi, Danilewsky and many others.

Another tributary consists of the ancient observations connecting the disease with marshes; the speculations of Varro, Columella, Lancisi, Beauperthuy, King, Laveran, Koch and Manson; and the valuable researches of those who tried to find the organism in marshes.

The third tributary consists of the early work on parasites; the discovery of metaxeny by Abildgaard (1790), Steenstrup (1842), and Küchenmeister (1851); the discoveries of Leuckart, Fedschenko (1858), Melnikoff (1868), Manson (1877), regarding certain worms; that of Smith and Kilborne (1889) regarding *Piroplasma;* of Bruce on trypanosomes; my work on human and avian malaria (1895-1899); and the confirmations and extensions which followed.

7. Recent History of Prevention.—It is open to question whether the extensive drainage works of the ancients had

been carried out for sanitary or for agricultural purposes; but there is no doubt that for several centuries the Italians, and other nations, have known how to control malaria by drainage and allied measures. In fact, for a long time the statement that *drainage reduces malaria* has been generally accepted as a medical dogma. As soon as the mosquito theorem began to become consolidated, the question arose whether the new knowledge would not provide us with some easier and cheaper method of prevention. My own studies had been undertaken principally for this object; and the following observations of mine were connected with it.

As early as 1884-1885 in Bangalore, and many times since then, I had noticed that *Stegomyia* and *Culex* could be largely reduced in numbers in my own house by emptying out the stale rain-water collected in tubs and pots in the garden. On one occasion I offered to reduce these mosquitos in the regimental mess-house, but the adjutant objected because, he said, I should be disturbing the order of nature! But I satisfied myself as to the possibility of making such a reduction by my own personal experiences in many parts of India.

After incriminating my dapple-winged mosquitos (Anophelines) in Secunderabad in 1897 I studied their habits there and in Kherwara, Calcutta, the Darjeeling Himalaya region, and Assam,[1] and noted the following points.

The eggs of the Anophelines were more or less boat-shaped, and possessed a peculiar membrane which gave them the appearance of having a row of oars on either side—thus differing from the eggs of the *Culex* and *Stegomyia*. They were also apt to arrange themselves in triangular patterns on the surface of the water. The larvae floated flat on the surface and had no long breathing tube—being thus unlike those of the other common groups. The adults were generally differentiated by having spotted (dappled) wings; a shape more suited for

[1] I could obtain no information on mosquitos in India at that time, not even at the Indian Museum in Calcutta.

long flight; and a peculiar attitude, when resting, at an angle to the surface to which they clung. These points would enable any one, even uneducated labourers, to distinguish them. But above all, my native assistants and myself noticed everywhere that the larvae do not generally breed in the pots and tubs occupied by those of the other groups, but mostly in *pools of water on the ground.* This led me at once (1897) to the explanation of the central fact that malaria is connected with marshes. If *Culex* and *Stegomyia* had been responsible for the disease it would not have possessed this relation—it would have been connected with pots and tubs, etc., just as yellow fever, which is carried by *Stegomyia*, is connected with them. I did not then know that the quartan and tertian parasites are also carried by Anophelines, but guessed, from the well-known relation referred to, that they were carried by some kind of marsh-breeding gnats, and not by the pot-breeding ones. The explanation was now clear; the ancients were quite right—the disease *is* caused by an emanation from the marsh. That emanation, however, is not a gas, nor even a *contagium vivum*, but an insect.

It was observed also that the Anophelines do not breed so much in large open bodies of water, such as lakes, rivers and reservoirs, but more in small, shallow, and often grassy pools and puddles where they can obtain shelter from wind and fish; and that like other mosquitos, they abound most in proximity to their breeding-places. Putting all these facts together, I reasoned that we now possessed a much cheaper and easier method of prevention. Previously, we had been obliged to drain a whole area at great cost; now we should be able actually to seek out and determine the exact malaria-producing pools, namely, those which contain the larvae, and then to fill up or drain these alone; and I naturally inferred that if the old method had been feasible (as it had been in many places), the new method would be still more easily feasible, and at less expense.

Before leaving India I described my proposals in a short report

to Government, dated the 16th February 1899, and published five months later without date, and with a title given apparently by the editor [1899]. The matter is so important, and my views have been so much misreported, that I must reiterate exactly what I said. I pointed out that mosquitos in general "are seldom to be found in the larger bodies of water"; that to get rid of them locally "it will suffice to empty out or drain away or treat with certain chemicals the small collections of water" in which they breed; that the Anophelines "seem to choose only rain-water puddles and ponds too large to dry up under a week or more, and too small or too foul and stagnant for minnows." I said that such pools "are not common in most parts of India except during the rains," and "seem to be so isolated and small that I think it may be possible to exterminate this species under certain circumstances." I added, however, that "I wish to be understood as writing with all due caution on these points." "I limit this statement to certain localities only, because it is obvious that where the breeding-pools are very numerous, as in water-logged country, or where the inhabitants are not sufficiently advanced to take the necessary precautions, we can scarcely expect the recent observations to be of much use—at least for some years to come. And this limitation must, I fear, exclude most of the rural areas in India. Where, however, the breeding-pools are not very numerous, and where there is anything approaching a competent sanitary establishment, we may, I think, hope to reap the benefit. . . . And this should apply to the most crowded areas, such as those of cities, towns and cantonments, and also to tea, coffee and indigo estates, and perhaps to military camps." "In making these suggestions I do not wish to excite hopes which may ultimately prove to have been unfounded." I concluded by urging further investigations on the malaria-bearing mosquitos of India and their habits. Though the report, being hurriedly written, might have been more exactly expressed in places, it is correct enough.

As it was the original statement of the radical anti-malaria measure—which has since been used with success in many parts of the world, and will certainly be adopted in the future as a fundamental principle of tropical urban sanitation—the reader should now clearly grasp what exactly was proposed. Animals tend to abound most where the local conditions are most favourable to them. Conversely, if we can make the local conditions unfavourable, they should become less numerous. Also, malaria being carried by certain Anophelines, should, as a broad general principle, be most common where they are most common, and should be reduced where they are reduced. I proposed, therefore, to reduce their numbers by measures directed against their breeding-places. But this proposal was obviously not meant to apply to the whole world, but only to places where the measures were most likely to be feasible—that is, generally to "crowded areas," and not to "rural areas." A square mile of town, containing thousands of ratepayers, may be assumed to have money available for such work; while open country, containing perhaps only a few scattered houses, has no such funds. Moreover, mosquito-reduction in a town will benefit thousands of people, while in the country it will benefit only a few—and that for the same cost. An identical principle applies to pipe water-supply and to pipe-sewerage systems—these are frequently given to towns, but not so often, even in Europe, to widely-scattered farmhouses. If then, I argued in effect, a town can afford to provide its inhabitants with a piped water-supply, or a sewerage system, it ought also to be able to keep them free, partially at least, from such dangerous insects as mosquitos. At that time I had had a rather exceptional experience of practical sanitation, having been specially appointed by the Government of India to improve the sanitation of a large Indian town (Bangalore), and I knew what I was writing about. As a general rule it would be much cheaper, I thought—and probably every practical engineer or health officer will agree with me—to make a great reduction

of mosquitos in most towns, than to instal and maintain a piped water-supply or sewerage system. I held also—and still hold—that the same measures might be feasible *in some cases*, even in plantations, military camps and villages.[1]

Unfortunately there are many people who seem to regard any new idea as a personal affront—who never try to understand the author, who disregard his reservations, and who attribute to him any absurd opinion! I was immediately accused of proposing to destroy every mosquito throughout India and Africa, and of trying to upset the order of nature, and so on. Still worse, many others accepted this idea without understanding the practical details, and did much harm by attempting the work in quite unsuitable places. Lastly, some rejected the idea, but pretended to test it experimentally—and of course failed.

In 1899, owing to the efforts of a number of medical men and men of affairs, and largely to the recent work on malaria, schools of tropical medicine were opened in Liverpool and London. At my inaugural lecture at the former (published at the same time as the previous paper) I repeated the same suggestions [1899]. Judging from my Indian experiences, I overrated somewhat the difficulty of reducing Culicines; but concluded by invoking experiments on such points.

West Africa, a great country hitherto paralysed by malaria, had long attracted me as an objective for practical preventive work, and I dreamed that it might be revolutionised by it. Hence, no sooner was the Liverpool School of Tropical Medicine opened, than I proposed to the Committee that I should be sent there to study the subject in detail. Consequently I left England in July (1899) with Dr H. E. Annett of the School, and Mr E. E. Austen of the entomological department of the British Museum, to see what could be done in Freetown, the capital of Sierra Leone—long known as the

[1] Malcolm Watson (section 57) is now extending Anopheline reduction to rural areas as well as towns. See also sections 42 and 43.

"white man's grave." As already stated (section 5) we quickly incriminated two local Anophelines, *P. costalis* and *Myzomyia funesta*, as carriers of malaria; and then set out to study their habits with a view to elaborating the principle of mosquito-reduction for practical sanitation.

Before we left it was arranged that our work should be published anonymously in the medical press as promptly as possible for the guidance of others; and I consequently wrote a series of four articles which were published in the *British Medical Journal* in September and October [1899.] The first two of them described the finding of the carriers; but the two latter (30th September, p. 869, and 14th October, p. 1033) gave the leading points in the habits of the Anophelines, the broad distinctions between them and the Culicines, the nature and distribution of their breeding pools, and other matters connected with the general theory of malaria, together with a plate showing the characteristic attitude, and one showing a characteristic breeding-pool. In all these points these Anophelines of Sierra Leone proved to be generally similar to those which I had studied in India. The articles drew wide attention to the subject; and in fact formed the basis of work on the reduction of mosquitos for malaria done since then; though, being anonymous, they are not often referred to in the literature of the subject.[1]

Being obliged to hurry back, we could spend only seven weeks in the Colony; and, as we were only private individuals, we could not carry out our recommendations. In February 1900 we published our report; in the main body of which I summed up my results and recommendations with plates and figures. Operations for reducing the Anophelines were divided into those meant to prevent their breeding, such as draining

[1] In 1899, G. H. F. Nuttall resuscitated some older works about mosquito-reduction. C. Finlay and A. F. A. King had recommended it hypothetically, and C. B. Aaron, L. O. Howard and others did practical experiments with oil, and other agencies; but these attempts (of which I was ignorant) do not constitute anything like the practical sanitary policy drawn up by me.

or filling the pools or treating them with Culicicides (oil, tar, lime, etc.); and the destruction of the adults or larvae. I devoted a whole section to discussing the local conditions under which such operations were likely to be successful, and concluded as follows:—

"(1) Operations against *Anopheles* are least likely to be effectual in level, water-logged localities, and in places where the insects breed in pools which cannot easily be found, or cannot easily be treated. (2) Operations will probably be easier in country which is not quite level, or where the rainfall is not great. (3) They promise to be very easy in extremely dry places. Lastly, it goes without saying that we can scarcely ever attempt to deal with *Anopheles* in large rural areas. On the other hand, we may reasonably hope to reduce them, if not to exterminate them, in the principal centres of population and civilisation—that is, just in the places where the prevention of malaria would be most useful—provided always that we make intelligent and persistent efforts to do so." Also, both in this report and in a previous small book of instructions for laymen [1899], I discussed *personal prophylaxis* by the use of mosquito nets, etc., and the reduction of Culicines round houses; and I also pointed out how much Europeans in Africa suffered from not being segregated from the natives as they are in India [February 1900, p. 45].[1]

I have described these thoughts and studies at some length, because they constitute the conclusion of my work commenced five years previously, long before others had considered the matter worth touching, and therefore have, I think, the right of priority. But, as I have already stated, immediately after the publications of my work in the middle of 1898, numbers of medical men and naturalists began to take up the matter in many parts of the world. The classification and habits of the

[1] I said also on the same page that a single intelligent native agent might "do a great deal of good" as regards the reduction of Culicines in many towns. On the strength of this I have recently been accused of proposing to rid the whole of Freetown of malaria by the services of one man only!

Anophelines and other mosquitos specially interested them, and innumerable papers and works on the subject began to be poured out. The entomological results were invaluable. Whereas in 1898 only about a hundred species of Culicidae were known, we have now recognised about six hundred, which have been carefully described in valuable works by E. E. Austen, G. M. Giles, L. O. Howard, R. Blanchard, and especially in an exhaustive monograph, by F. V. Theobald, and by many others. Regarding the habits of the insects my findings have been generally confirmed, without very much really new matter being added. Some of the observers, being new to the subject, have laboriously recorded facts (such as the abundance of gnats near their breeding-places, or in ground-floors of houses, or in dark corners, or on dark surfaces, or their transportation by carriages, and so on) which, I think, were perfectly familiar to all who have lived much amongst them.[1]

Returning to the subject of prevention, we must now note that an important new method was suggested by R. Koch during his visit to Italy in 1898—namely, prevention by treatment of cases. This was tried at once by B. Gosio in Grosseto in Italy [1900]. Towards the autumn of 1899 Koch went with R. Pfeiffer and H. Kossel to Batavia, where he discovered his law regarding the frequency of infection in native children; and in December proceeded to German New Guinea, where he successfully used his method of prevention at Stephansort [1900]. This method is entirely different to the ancient one of drainage, or to my modification of it. It aims, not at the reduction of the carriers of the parasites in a locality, but at the reduction of the parasites themselves by the general and complete treatment by

[1] Also many habits have been ascribed to mosquitos, which exist only in the imagination of the writers. Thus Nuttall says [1899] that Aaron suggested destroying mosquitos by placing small lamps in trays of petroleum over ponds or marshes round dwellings. The gnats, attracted by the light, would fall into the oil. In 1900 two Italian writers, one of whom is supposed to have led the way regarding the mosquito malaria theory, not only approved this absurdity, but added the suggestion that the lamps "be furnished with powerful (*sic*) reflectors turned away from the house." I have never in my life seen mosquitos attracted by lamps.

quinine of all infected persons in the locality. If this is done, he argued, the Anophelines, however numerous they may be, will be innocuous, as they will find no parasites to carry. He describes the method ably, and says: "After all these experiences I consider myself warranted in stating that we are in a position, by means of the procedure which I have described, to make every malarious region, according to circumstances, wholly or nearly free from malaria." The method is invaluable, and there is no real rivalry between it and my method.

Most admirable, too, has been the work in Italy, led by A. Celli, himself one of the most distinguished students of malaria. In 1899 he published an excellent text-book, in which he presented the whole subject of prevention according to the "new researches," with a history and accounts of various preventive measures. This was followed by innumerable researches and practical campaigns throughout the country—which have made a great reduction in the disease. All methods have been adopted, but principally perhaps the various quinine methods. Italy, however, is a temperate European country, with conditions very different to those which prevail in the tropics—and the malaria there is mostly rural.

Still another method consists in the screening of houses by wire-gauze, a defence long employed in America against mosquitos. It was now used in Italy against Anophelines; was much encouraged by the experiment of L. Sambon and G. C. Low in the Campagna in 1900; and has been widely employed at Panama.

In 1898, the Colonial Office and the Royal Society appointed a commission, consisting of C. W. Daniels, J. W. W. Stephens and S. R. Christophers, to follow up my work. Daniels left the commission in 1899 after some good work in British Central Africa; but the other observers were sent at my recommendation to West Africa during the same year. There they continued our researches of 1899, but laid special

stress on *segregation* for Europeans. They found many interesting facts about mosquitos, and also developed very fully the subject of infection of native children, and the causes of local malaria in West Africa. In 1901 they were sent to India at my suggestion, and continued similar work there. Their valuable observations are contained in a series of reports [1899-1903].

Also following our first expedition to Sierra Leone, the Liverpool School of Tropical Medicine sent out a stream of expeditions, all of which gave useful information about local malaria, or started practical measures.

At the end of 1900 a very important event occurred—W. Read, J. Carrol, J. W. Lazear and A. Agramonte proved in Havana that yellow fever is carried by *Stegomyia calopus* (*fasciata*) just as malaria is carried by Anophelines. They proved this by direct experiment on men; but did not succeed in finding the yellow fever organism either in man or in the mosquito. Early in 1901 General Leonard Wood instructed the Chief Sanitary Officer of Havana, then Major M. C. Gorgas, to commence operations against these insects. In a few months the disease was banished from Havana.

It was very disappointing at this time that so little had yet been done in British possessions to combat malaria by any of the measures rendered possible by the previous discoveries. There was a brilliant exception. On my visit to Sierra Leone I had met Dr H. Strachan, the Principal Medical Officer of Lagos, and he took up the matter with great interest. Fortunately the Governor of Lagos, Sir William MacGregor, himself a medical man (like General Leonard Wood), immediately saw the value of the new ideas, and began to push malaria prevention in Southern Nigeria in a most admirable way by all methods—drainage, quinine, wire-gauze and public instruction. But elsewhere local bodies had ignored the subject almost entirely. Even in Sierra Leone, where the way had been pointed out exactly, and where the disease was

the principal enemy of the colony, little had been done. The Acting Governor, now Sir Matthew Nathan, who had been very sympathetic during our visit in 1899, had left for the Gold Coast; and, beyond appointing a single person for a few months to check the mosquitos, the municipality of Freetown had dropped the matter. Calcutta, where also I had suggested an anti-mosquito campaign in 1899, had done the same. Everywhere, my proposal to reduce mosquitos in towns was treated only with ridicule.

Early in 1901, therefore, I determined to try and give an object lesson in the work recommended by me. A rich gentleman of Glasgow kindly offered me £2,000 to defray expenses; and others helped me in various ways. On the 2nd July I arrived with Dr M. Logan Taylor, at Freetown, and, with the support and approval of the Governor, Sir Charles King Harman, commenced the work at once. Since 1899 I had gradually become convinced that anti-mosquito campaigns had better be conducted, not only against Anophelines, but against all kinds of mosquitos at once; and this opinion was strongly reinforced by the yellow fever discovery just made, and also by the fact that certain Culicines carry filaria. In fact, since then I have always maintained the opinion which may be put as follows: *It is one of the first sanitary duties of all municipalities and town councils in warm climates to keep down as much as possible the numbers of mosquitos within the area placed under them.* We therefore engaged the services of twenty men, to whom the Governor added twelve; and with these we removed piles of mosquito-breeding rubbish from the back yards of houses, and drained, emptied, filled up or oiled numbers of Anopheline pools. I returned to my duties in Liverpool on the 16th August; but M. Logan Taylor continued the work. In September C. W. Daniels of the London School of Tropical Medicine arrived in Sierra Leone, and reported favourably on the work; and in January 1902 I visited it again for a week to watch progress,

There is no doubt that a great reduction in the number of mosquitos was effected by our measures; but, of course, our work was meant only for a temporary object lesson, and we stated clearly that the improvement would last only if our measures were continued by the local authorities. Later in 1902, however, Logan Taylor reported that these authorities did not appear very anxious to continue them; and we consequently closed the work when our money was expended —having done, we thought, as much as private persons could do.[1]

After my third visit to Freetown, Prince August d'Arenberg, President of the Suez Canal Company, asked me to advise regarding the prevention of malaria at Ismailia on the Canal. I arrived there in September 1902 with Sir W. Macgregor, who did me the honour to accompany me. The town was swarming with both Culicines and Anophelines; and efforts to control the malaria by liberal quinine distribution had not succeeded. I advised a continuance of this measure with general mosquito-reduction. In this case "intelligent and persistent efforts" were made; and within a year the insects of both groups were almost entirely banished, and the admissions for malaria reduced from about 2,000 a year to 214. Since then, endemic malaria has entirely disappeared. Dr A. Pressat says that he originally reduced the disease by means of a mosquito brigade of only four men *qui a tout fait* [1904].

Scarcely less brilliant was the campaign at Klang and Fort Swettenham in the Federated Malay States, commenced in 1901, and carried out chiefly by Dr Malcolm Watson. At Hong Kong also, a campaign was commenced at the same time, and carried out chiefly by Dr J. C. Thomson with very good results. Several anti-mosquito campaigns have been conducted in the United States; and in 1906-1907 my brother, E. H. Ross, acting under the orders of Sir Horace Pinching,

[1] A year later it was pretended that our efforts had failed, and the only thanks which we ever received for our pains was a subterranean effort to injure us.

completely cleared Port Said of Culicines. Other campaigns have been those of Dr A. Balfour at Khartoum, of many officers in Candia, some West Indian Islands, the East Indies, and Durban. In 1906 I went to Greece to advise regarding malaria at Lake Kopais; and in 1907 to Mauritius (with Major C. E. P. Fowler) to advise the Government on malaria prevention there. As regards magnitude, however, the great American campaign, under Colonel Gorgas, against both malaria and yellow fever, at Panama; that of Oswaldo Cruz at Rio Janeiro; and that, so well guided by Drs Ed. and Et. Sergent in Algeria, have long held public attention. Descriptions will be found under the appropriate sections in this book.

In spite of these brilliant examples, however, little was attempted against malaria for many years in the vast bulk of tropical countries. It is true that questions in Parliament and elsewhere often led to assertions that much was being done; but those who really know what might have been done, and who were in a position to obtain the information from men on the spot, remained far from satisfied. On 23rd April 1906 the Earl of Elgin, Secretary of State for the British Colonies, issued a circular at the instance of Colonel Seely, M.P., to all Governors of Crown Colonies asking for specific details on this point. In September of the same year I read a paper at the 14th International Congress of Hygiene at Berlin [1907] in which I described what had been done in some tropical localities, and also criticised the lack of efforts in others. Still, however, little effect was produced, and I came to the conclusion that in many malarious places the authorities had no real desire to carry out anti-malaria measures, or else were entirely ignorant of how to do so. For example, though they always urged lack of funds as an excuse, they often omitted to do things which would cost almost nothing, such as to undertake the measurement of malaria, to issue careful reports, or to form the nucleus of an organisation.

At the end of February I visited the Medical Congress at

Bombay in order to ascertain what work was being done in India, but met only with disappointment. A few months later, therefore, I gave a lecture at the Royal Institution of Great Britain, in which I ventured to complain very strongly regarding this state of affairs [1909]. Fortunately this has been followed by a considerable improvement. An anti-malaria league was started in South Africa, and the Indian Government held a conference on malaria at Simla, which practically endorsed all our teaching. Moreover, other movements for the generalisation of anti-malaria measures are now on foot, and will, I hope, produce better results.

8. **Remarks**.—Such in brief is the august history of malaria—and some day it will be considered as being no unimportant part of the world's history. To the thoughtful student of human affairs it contains many points of interest.

We should note in the first place that several methods of prevention were well known long before the mosquito-theorem was established. Probably the ancients, and certainly the Italians, since the time of Lancisi (1717), were so well acquainted with the good effect of drainage that this principle was accepted as a dogma in all the medical schools. The excellent results of quinine, both for cure and prevention, have also been well known, at least since the time of Maillot in Algeria (1834)—so well known in fact that the Government of India has sold the drug for many years at the public post-offices for a small price. Even the benefit of using a mosquito-net during sleep was, I remember, looked upon as an elementary doctrine when I first went to India in 1881. The mosquito-theorem has merely verified and precised these old ideas.

It was therefore I think possible, long before 1899, to reduce the disease anywhere on a large scale. Efforts in this direction would have been invaluable in numbers of settlements in Africa and in the East and West Indies; but were, I believe, seldom if ever made. Certainly many intelligent medical men

advocated them; but there is a wide interval between the suggestion of public health measures and their execution. Where they were commenced they were soon abandoned; no continuous public policy against the disease seems ever to have been adopted; settlements were left for years at the mercy of marshes known to be poisonous; and the methodical distribution of quinine remained undreamed of. I have little doubt that if more intelligence had been shown in these respects many of these settlements would have already grown into prosperous states.

Laveran's discovery of 1880 was one not only of theoretical, but of practical, importance. It enabled medical men to distinguish with certainty cases of malarial fever from other diseases, and, after making the diagnosis, to treat the patient methodically. But many years elapsed before it began to be used for these purposes. The parasites were not seen in India, for example, until 1887 (by Vandyke Carter). I did not hear of the discovery until 1888, and did not see the parasites until 1894. Scarcely half a dozen men were studying them in India at that time. The use of the microscope scarcely became general anywhere in the tropics until after 1900; and quite recently medical men have told me of hospitals which were not even provided with these indispensable instruments. Numbers of others had never read the fundamental literature; and up to the present day many colonies possess no sufficient organisation for circulating recent medical papers. Yet in most tropical countries there were large public medical services which ought to have been properly provided for in these respects.

It is surprising, when we remember that malaria often causes a third of the sickness in a place, to note that Laveran's discovery was not immediately followed by public investigations on the subject, especially as to the route of infection.[1] The work was done almost entirely by private persons. Even when

[1] In 1898 the Government of India appointed me to undertake researches on malaria—this being, I believe, the first case of the kind.

information was thus obtained, it was left for years untaught in the medical schools, or neglected in practice. But worse than this, little has been done even to encourage the private workers. Indeed I know of several cases where such workers have received for their pains nothing but the ridicule of less laborious colleagues, and the persecution of jealous superiors.[1]

There was no reason why the prevention of malaria according to the new methods should not have been commenced after 1899 in every civilised malarious country. Such countries possess responsible governments and expensive sanitary and medical services—in fact all the machinery required for the work. But the efforts actually made have been due almost entirely to isolated enthusiasts, while in many places nothing approaching a genuine campaign has even yet been attempted. In others, again, some efforts have been reported, but their perfunctory nature is only too apparent to those who are acquainted with the subject. Yet the work has been done, and well done, in many places. Why then has it not been done in all?

The facts are undeniable. In numerous localities the disease causes not only an immense amount of sickness, but a great loss of efficiency amongst officials, soldiers, workmen and the general public, and a corresponding waste of public money and reduction of prosperity. It can be combatted only by well-organised government action; and governments admit their responsibility in such matters. Yet governments have taken little part in the investigation of malaria, and have even neglected to utilise for practical purposes the investigations of others. Nor is the case of malaria the only one of the kind. Filiriasis is very prevalent in many localities, where it produces numerous unpleasant diseases; yet P. Manson's discovery of 1877, that the parasite is carried by mosquitos, has scarcely ever been utilised up to the present (except perhaps in the case

[1] The treatment of my brothers in Egypt is well known. In British administration we do not expect gratitude for scientific work; but is there anything more childish than to punish such labours by gratuitous persecution?

of G. C. Low's work at Barbados). Another example (and I could quote many) is that of the entry of plague into India in 1896—an event which has destroyed millions of lives and wasted millions of money. Precisely similar complaints are frequently heard regarding diseases of animals and of crops, and regarding mining, manufactures and commerce.

The student will do well to ask himself the cause of this phenomenon—which is of fundamental importance in connection with our subject. We recognise, of course, that the world is by no means perfect; but for this very reason we should endeavour to extricate the causes of its imperfections. The following points may be suggested for consideration, or correction, by the reader.

The principal cause of the faults referred to seems to me to lie in a curious tone of thought which is now very prevalent, especially in this country; according to which all matters of practical utility to mankind are looked upon as being rather base preoccupations for the more perfect type of human being. The study of nature, the extirpation of disease, the management of cities, invention, the development of agriculture, manufactures and commerce, and the organisation of prosperity are inferior things. We are to seek a higher level in personal manners, literary criticism, eloquence, sport, party politics, sectarian dogma, and so on. It is the modern equivalent of Indian fakirism, by which, lost in various lofty speculations, we are taught to remain content in the midst of starvation and disease.

This spirit, fostered in us from childhood, leads to false ideas of government. The scientific side of administration is apt to be forgotten in the noise of endless and despicable party strife; progress ceases while we discuss abstract notions about law, liberty, representation, nationalism and so forth; the machine refuses to work while the mechanics are quarrelling over the lubrication. The result is precisely what may be imagined—cities built without sense or forethought, filthy slums, hovels filled with disease, gulfs of destitution; and the voices of

those who would better this state of affairs by scientific methods are lost among the yells of the opposing factions.

Though in the tropics we are still spared many of the evils of party government, the same spirit of inutilitarianism often remains. Money which would suffice for the assainment of whole towns is wasted rather on the construction of extravagant town halls and post-offices. The cost of a new hospital would often suffice to prevent more cases of sickness than are treated in it; and that of invaliding to prevent the disease which causes it. The British Government spends £15,000,000 a year on the education of children; but gives, I believe, literally nothing for the scientific investigation of scarlet fever, measles, whooping-cough, chicken-pox and mumps, which work such mischief among these same children — a fact which by itself proves the illogical nature of the education given. Similarly in the tropics I have seen a large class of coloured children, almost all of whom were suffering at the time from fever or enlargement of the spleen, being taught the dates of accession of the Plantagent kings! We all know how often sanitation receives only the crumbs which remain at the bottom of the public pocket after all the other departments have been well fed—the final triumph of fakirism.

But we must not blame the general public alone. We are familiar with the manner in which individual medical men give their services, not only to medical science, but for the gratuitous treatment of the poor; yet nevertheless the medical profession as a body possesses little influence or power in public affairs. It is a body without a head. It does not take a high enough stand with the public regarding scientific and sanitary matters. The duty of the profession does not lie merely in teaching and in the cure of the sick, but in everything that appertains to the health of the people. Yet it is apt to be dominated, not by this lofty ambition, but by other ideals; and it therefore seldom bestirs itself regarding

its higher duties. Can we wonder then that governments also are apt to neglect the same duties?

Several high officials have told me that medical men do not often become good administrators. This is probably due to the fact that governments seldom take enough care in the selection of their administrative medical officers. I have seen many able men in such posts, but others who were not fitted for them. The head of a large sanitary or medical department should possess a full knowledge of the recent scientific and practical advances in his subject. Too often we find officials who have been promoted merely on account of seniority, or in consequence of self-service; men who express contempt of these recent advances because they know nothing about them; who have no ideas, and who make no efforts. Such men, when taxed with defects in their department, always complain that the Government does not grant enough funds. But they are the responsible heads of those departments; and if their advice is neglected they should resign their post.

In 1730 Dr Thomas Fuller (*Exanthematologia*, Charles Rivington, London—quoted by W. S. Thayer) wrote: "Can any man, can all the Men in the World, tho' assisted by Anatomy, Chymistry, and the best Glasses, pretend positively and certainly to tell us, what particles, how sized, figured, situated, mixed, moved, and how many of them, are requisite to produce a quartan ague, and how they specifically differ from those of a tertian . . .?" We are now able to tell all these things. They have been written in hundreds of books, and are familiar to thousands of students. Those who belittle the powers of science are not always, perhaps, the wisest of men.

The history of malaria contains a great lesson for humanity—that we should be more scientific in our habits of thought, and more practical in our habits of government. The neglect of this lesson has already cost many countries an immense loss in life and in prosperity.

CHAPTER II

SUMMARY OF FACTS REGARDING MALARIA

Suitable for Public Instruction

9. The Parasites and the Fever. — Malaria, or Malarial Fever, is also known by the names Paludism, Marsh Fever, Jungle Fever, Ague and Periodic Fever. It is often called by local names such as Country Fever, West African Fever, Burma Fever; and varieties of it are called Intermittent Fever Remittent Fever, Pernicious Fever and Blackwater Fever.

Malarial fever occurs more or less in all warm climates, especially in the summer, after rains, and near marshy ground; and causes a quarter or more of the total sickness in the tropics.

It is caused by enormous numbers of the minute parasites of the blood called *Plasmodia.*

These parasites are introduced into the blood through the proboscis of certain species of the mosquitos called Anophelines.

On being introduced, each parasite enters one of the red corpuscles of the blood, in which it lives and grows.

On reaching maturity each parasite produces a number of spores which escape from the containing corpuscle, and enter fresh corpuscles; and this method of propagation may be continued indefinitely for years.

Thus, though only a few hundreds or thousands of the parasites may have been originally introduced through the mosquito's proboscis, their number rapidly increases until as many as some millions of millions of them may exist in the blood.

At first, while the number of parasites is still small, the

infected person may remain apparently well. When, however, the number is large enough, he begins to suffer from fever.

The parasites tend to produce their spores all at the same time; and it is at the moment when these spores escape that the patient's fever begins.

The fever is probably caused by a little poison which escapes from each parasite with the spores.

After from six to forty hours or more this poison is eliminated from the patient's system; and his fever then tends to leave him.

In the meantime, however, another generation of parasites may be approaching maturity, and may cause another attack of fever like the first; and so on, indefinitely for weeks or months. In this manner the attacks of fever follow each other at regular intervals.

But it often happens that before one attack has entirely ceased another one commences; so that the attacks overlap each other, and the fever is continued.

After a time, even without treatment, the number of parasites may decrease, until not enough of them are left to produce fever; when the patient improves temporarily.

It generally happens, however, sooner or later, that the number of parasites increases again; when the patient again suffers from another series of attacks.

Such relapses are frequently encouraged by fatigue, heat, chill, wetting, dissipation and attacks of other illness.

They may occur at intervals for a long time after the patient was first infected, and after he has moved to localities where there is no malaria.

It is probable that as long as one parasite remains alive in the patient's blood he may remain subject to such relapses.

Besides fever, the parasites often produce anaemia and enlargement of the spleen, especially in patients who have suffered from many relapses.

Death is sometimes caused by sudden and grave symptoms.

Chief among these are the symptoms known as Blackwater Fever, or Haemoglobinuria, which generally occurs in old and neglected infections.

Death is also often caused during the course of a malarial infection by other diseases, such as pneumonia or dysentery, acting upon a constitution already enfeebled by the parasites.

If the patient survives, the parasites tend to die out of themselves, without treatment, after a long period of illness—leaving him more or less "immune."

The parasites are at least of three kinds, which can easily be distinguished in blood placed under the microscope. These are (1) the parasite which produces its spores every three days and causes *quartan fever*; (2) the parasite which produces its spores every other day, and causes *tertian fever*; (3) parasites which cause the so-called *malignant fever*, in which dangerous complications most frequently occur.

If a little blood containing any one of these species of parasites is taken from a patient, and is then injected into a vein of a healthy person, the latter will almost certainly soon become infected with the same species of parasite.

Closely similar parasites are found in monkeys, bats, squirrels and birds.

As proved by centuries of experience, cinchona bark, from which quinine is made, possesses the power of destroying the parasites and curing the infection. But it will not generally destroy all the parasites in the body unless it is given in sufficient doses and continued for several months; and as long as a single parasite remains alive in the blood, infection is continued and the patient may be subject to relapses. At least five grains (1/3 gramme) of sulphate of quinine should be taken by an adult patient every day without fail for four months; but he should consult a medical man regarding details of the treatment.

10. The Mode of Infection.—Besides those forms of the

malaria parasite which produce spores in the human body, there are other forms, male and female.

When certain species of the mosquitos called Anophelines happen to feed on a patient whose blood contains the parasites of malaria, these are drawn with the blood into the insect's stomach.

If the sexual forms of the parasites are present these undergo certain changes in the mosquito's stomach; the females pass through its wall; and finally fix themselves to its outer surface—that is, between the stomach and the skin of the insect.

In this position they grow largely in size, and after a week, in favourable circumstances, produce a number of spores.

The spores find their way into the insect's salivary glands. This gland secretes the irritating fluid which the insect injects through its proboscis under the human skin when it commences to feed; and the spores can easily be found in the fluid by the microscope.

Thus when a proper species of Anopheline, which has more than a week previously fed upon a patient containing the sexual forms of the parasites of malaria, next bites another person, it injects the spores, together with its saliva, under his skin—that is, generally into his blood.

These spores now cause, or may cause, infection or re-infection in this second person, as described at the beginning of this summary.

Numerous birds and men have been infected experimentally in this manner.

Thus the parasites of malaria pass alternately from men to certain mosquitos, and back from these mosquitos to men.

A very large number of parasites are known which pass in this manner from one animal to a second animal which preys on the first; and back again from the second animal to the first.

It is not known with certainty when and how this process first commenced; but probably all such parasites were originally free living animals, which by the gradual evolution of ages acquired the power of living in other animals.

Thus also, it is evident that malarial fever is an infectious disease which is communicated from the sick to the healthy by the agency of certain mosquitos.

From the time of the ancients it has been known that malarial fever tends to be most prevalent in the vicinity of marshes.

The parasites of malaria have never been found in the water or air of marshes; nor in decaying vegetation; nor in the soil. Attempts to produce infection by these have always failed. But the Anophelines which carry the parasites breed in marshes and in marshy pools and streams.

Rising from these marshes, they enter the adjacent houses and feed on the inmates, mostly at night; biting first one person and then others; and living for weeks or months.

If an infected person happens to be present in any of these houses, the infection is likely to be carried by the Anophelines from him to the other inmates, and to neighbouring houses.

Thus the whole neighbourhood tends to become infected, and the locality is called "malarious."

In such localities, it is easy to find the parasites of malaria in the Anophelines of the proper species; even in as many as 25% or more of them.

Such Anophelines when taken from a malarious locality to a healthy one (*e.g.*, from the Campagna near Rome to London) will still infect healthy persons whom they have been caused to bite.

So also, in malarious localities, the Anophelines bite the healthy new-born children, and infect many of them.

Such children, if not thoroughly treated, may remain infected for years; may become anaemic and possess enlarged

spleens; and may spread the infection to others. Later, however, at the age of twelve years or more, the survivors tend to become "immune."

In many malarious localities almost every child has been found to contain the parasites of malaria, or to possess an enlarged spleen.

In such a locality therefore, the infection is constantly passed on from the older children, or from adults, to the new-born infants; so that the locality may remain malarious for years, or for centuries.

Similarly, a new-comer arriving in such a locality is very likely to become infected, especially if he sleeps in an infected house even for one night.

A locality is said to be malarious only when healthy persons become infected in it; not when persons who have become infected elsewhere happen to reside in it.

A locality is malarious only when it contains persons already infected with the parasites, and also sufficient numbers of the proper species or varieties of Anophelines to carry the infection to healthy persons.

The chances of infection tend to be great in localities where there are already numerous infected persons, not treated with quinine; or where there are numerous Anophelines of the proper species, not prevented from biting.

Conversely, the chances of infection tend to be less where infected persons are excluded, or properly treated with quinine; or where the Anophelines are few in number, or are prevented from biting.

11. Facts about Mosquitos.—Gnats, which in the tropics are commonly called mosquitos, belong to the zoological family of Insects known as the Culicidae (from the Latin *culex*, a gnat). They are distinguished from other insects by a number of characters; and always possess only one wing on each side, and a long proboscis.

Like that of other insects, their life is divided into four stages, the *egg*; the *larva* (or caterpillar); the *pupa* (or chrysalis); and the *imago* (or adult, winged insect).

The *egg* is laid on water or near it, and in warm, moist weather hatches out in a day or two.

The *larva* is entirely aquatic, and always lives in water. It swims and dives by means of paddles and hairs, and feeds on various aquatic organisms. It cannot, however, breathe under water, but must always rise to the surface in order to obtain air. After a week or more it becomes a pupa.

The *pupa* still remains in the water, generally floating on the surface. After two days or more its skin cracks, and the imago emerges.

The *imago* remains standing for a little while on the empty floating skin of the pupa, and then flies away.

Both males and females are able to suck fluids through the proboscis. As a rule the male feeds only on the juices of plants; but the female sucks the blood of men, beasts, birds and reptiles. The female often returns to water every few days in order to lay her eggs, of which she may deposit several hundreds at a time; and then seeks another meal.

Female gnats have been kept alive in captivity for months.

In unsuitable weather both males and females may take refuge in damp places such as cellars, wells, out-houses and woods, where they may remain for months until better conditions prevail.

As a rule gnats, like other animals, tend to remain in the locality where they were born; but a few may occasionally stray to the distance of half a mile or more from their breeding places. If, however, plenty of places where they can obtain food exist near at hand, there is no reason why they should travel further for it. They must also remain near water to drink and to lay their eggs in.

Gnats are generally favoured by warm weather, by plenty of water suitable for their larvae, and by abundance of food.

They tend to be diminished by various kinds of bats, birds, fish, insects and spiders which devour them or their larvae.

During its life, a single gnat may succeed in biting many persons or animals, and in propagating diseases amongst them.

The family of Culicidae or Gnats is divided into many subfamilies and genera, and contains some five or six hundred known species.

Although all these species have many habits and structural characters in common, yet they all differ in small details. These have been described at length in a number of special books written on the subject.

In the tropics, as a broad general rule, the gnats which most concern human beings belong to the groups called *Culex*, *Stegomyia* and *Anophelina*.

Culex pipiens is a very common gnat in Europe, and allied species are found almost everywhere in the tropics. The larvae occur principally in tubs, barrels, cisterns and other vessels containing water, in stagnant ditches, garden pits, holes in rocks and trees, and so on. They possess a long breathing tube close to the tail fins; and float at the surface of the water with the end of this tube attached to the "surface film," and the head hanging downwards. When disturbed, they wriggle at once to the bottom. The adult insects generally present a uniform grey appearance, with pale yellowish bars across the back of the abdomen, and plain unspotted wings. They bite almost entirely in the evening and night, and principally indoors (in the tropics). The parasite which causes elephantiasis, namely the *Filaria bancrofti*, is carried by them or allied species in a manner very similar to that in which the Anophelines carry the parasites of malaria.

Stegomyia calopus and allied species are very common in the tropics, but much less so in temperate climates. The larvae breed in much the same places as those of *Culex*, but more frequently in vessels. Any old biscuit tin or oil tin, flower-pot, broken bottle and crockery, tub or barrel, choked drain, roof

gutter, etc., in which rain or other water has collected, is almost sure to contain them, and they frequently occur in holes in trees and in certain plants. They possess a short stumpy breathing tube, and float head downwards, like the larvae of *Culex*. The adult insects are more or less striped or speckled black and white, and have plain unspotted wings. They bite chiefly in the daytime, and often abound in woods and in the shade of trees. In America *Stegomyia calopus* carries yellow fever.

The Anophelines consist of about one hundred and twenty known species, some of which carry malaria and are always found in malarious places. The larvae occur chiefly in water on the ground, particularly in water which stands or flows amongst grass or water weeds. Thus they abound in the weedy margins of rivers, streams, lakes and ponds; in small sluggish streams and streamlets; in water courses, drains and gutters choked with weeds; in pools of rain water lying on grass; in pits from which earth has been removed, such as the "borrow pits" by the side of railway embankments; in cisterns and pits used for watering gardens; in ornamental waters; in hollows in rocks; and in water at the bottom of boats, etc. Thus while the larvae of *Culex* and *Stegomyia* occur in the small collections of water which abound in and around houses, on the other hand the Anophelines are principally *marsh mosquitos*—owing to which malaria is generally more or less connected with marshy conditions, and is called Marsh Fever, or Paludism. The larvae feed chiefly on the surface of the water—on which they float like sticks, and not with the head hanging downwards. When disturbed they swim away backwards on the surface, and sink to the bottom only when much alarmed. They have no breathing tube, like those of *Culex* and *Stegomyia*. The adults are speckled brown and white, or black and white; and generally the wings are not plain, but possess three or four black marks along or near the front border. When the insect is seated at rest on a wall, the tail projects outward at an angle from the

wall; whereas the *Culex* and *Stegomyia* sit with the tail hanging downwards, or even nearly touching the wall. All these facts enable any one to distinguish at sight both the larvae and the adults of Anophelines from those of *Culex* and *Stegomyia*. Anophelines bite chiefly at night or in the dusk — owing to which the malarial infection is generally acquired at night. They enter houses, but also bite in the open in spots sheltered from wind.

12. Personal Prevention.—If they can avoid it, people should not go to live in known malarious places, nor in the vicinity of marshes, nor close to an infected native population.

Even in such, however, the chances of infection can be much reduced by the careful use of mosquito nets. The net should not have the smallest hole. It should be hung *inside* the poles, when these are provided, and not outside them. It should be tucked under the mattress all round, and should never be allowed to hang down anywhere to the floor; and it should be stretched tight, in order to allow every breeze to enter, and should not be hung in loose folds, which check ventilation.

Those who can afford it should protect the windows of the house with wire gauze, and provide the doors with automatic closing arrangements. It is especially advisable to protect a room, or a part of the veranda, for sitting in during the day or evening.

Punkas and electric fans, not only drive away mosquitos, but also keep the body cool, comfortable and vigorous, even in great tropical heat.

Where there is great danger of malaria, five grains (1/3 gramme) of quinine should be taken regularly every day just before breakfast; but it is advisable to take a double dose at least once a week—say, on every Sunday.

In such localities, the hands and feet may also be protected by gloves and boots; but these cannot always be endured owing to the heat; and it is preferable instead to carry and use

constantly a palm-leaf fan, with which mosquitos can be driven off and the body kept cool.

A small butterfly-net of white (not green) muslin may be kept in the house for the purpose of catching troublesome mosquitos during the day; and the servants may be taught to use it. It may sometimes be easier to kill all the mosquitos in a room in this manner than by fumigation.

Mosquito traps consist of boxes lined with black cloth. Attracted by this colour mosquitos enter the box for refuge during the day. The lid or shutter is then suddenly closed and the insects within are killed by a little ammonia or chloroform poured into the box through a protected opening. Or, the lid can be made to slide down to the bottom of the box in such a manner as to crush the inmates.

To fumigate a room thoroughly for mosquitos all the chinks in the doors and windows should be closed by pasting paper over them. Then burn the culicide as follows (Sir Rubert Boyce):—

1. *Sulphur.*—Allow 2 lbs. of sulphur to 1,000 cubic feet. Use two pots, place them in a pan containing 1 inch of water to prevent damage, and set fire to the sulphur by means of spirit.

   Duration.—Three hours.

2. *Pyrethrum.*—Allow 3 lbs. to 1,000 cubic feet, and divide amongst two or three pots, using the same precautions as with sulphur.

   Duration.—Three hours.

3. *Camphor and Carbolic Acid.*—Equal parts camphor and crystallised carbolic acid are fused together into a liquid by gentle heat. Vaporise 4 ozs. of mixture to each 1,000 cubic feet; this can be done by placing the liquid in a wide shallow pan over a spirit or petroleum lamp; white fumes are given off. To avoid the mixture burning, the fumes should not come in close contact with the flame of the lamp.

   Duration.—Two hours.

Remember that sulphur tarnishes metal work and injures pianos, sewing machines, chronometers, telephones, etc.

The camphor-carbolic mixture is one of the most agreeable and effective of the various agents.

Many mosquitos may be killed, without troubling to cover the chinks with paper, simply by picking up and destroying the stupefied insects near the windows after the fumigation.

The householder should always take care that no stagnant water is allowed to remain anywhere in his premises in cisterns, drains, gutters, tubs, jugs, flower-pots, gourds, broken bottles and crockery, old tins and other rubbish, or in holes in trees, or in certain plants such as wild pineapple — in all of which mosquitos are apt to breed. If the water cannot be emptied out, the larvae of the mosquitos in it may be destroyed by pouring a little kerosine oil, or in the case of drinking water, a little eucalyptus oil, upon the surface. This should be done once a week; and the householder should make it a habit to inspect his premises for this purpose every Sunday. Cisterns and tubs containing drinking water should be kept screened in such a manner as to prevent mosquitos laying their eggs on the surface of the water. If he follows this advice the householder will generally obtain a great reduction in the number of the insects in his house. If, however, the plague continues, he should appeal to the local sanitary authorities.

It is extremely dangerous to sleep in a house which is occupied, or has recently been occupied, by infected persons, especially native children; or in or close to an infected native village.

13. Public Prevention.—As a broad general rule, malaria causes a quarter or more of the total sickness in the tropics.

Malaria can always be greatly reduced, or may even be extirpated, in any locality.

Large marshes in populous places must be drained, deepened or filled up.

A proper permanent organisation must be established for dealing with the smaller breeding-places of Anophelines, and for distributing quinine—especially to infected children. Other measures may be adopted where called for.

The cost is likely to be more than recouped by saving in life, labour, invaliding, medical attendance and hospital accommodation.

The campaign will remove other mosquito-borne diseases as well as malaria; and will tend to improve general sanitation in the locality where it is undertaken.

CHAPTER III

THE FUNDAMENTAL OBSERVATIONS AND EXPERIMENTS

14. The Parasites cause the Disease. — Having studied the history and a general outline of our subject, we must now examine the fundamental observations and experiments in greater detail. It is advisable to begin with the evidence which proves that the parasites are really the cause of the disease.

(1) *The parasites are not found in those who have not suffered from malarial disease.* Laveran discovered the parasites of malaria at Constantine, Algeria, in 1880. Since then the blood of hundreds of thousands, possibly of millions, of patients suffering from various diseases must have been examined, during the ordinary course of clinical work, in the civilised countries of the globe. Yet I have never heard of a single case in which Laveran's parasites are reported to have been found by a competent observer in persons who have never had malarial fever. For example, in Britain, which is now practically non-malarious, they are never seen in the vast numbers of hospital patients. In only two cases that I know of have they been observed in men who have never been out of this country, and both these patients suffered from the characteristic symptoms of malarial fever.[1] In the tropics they are often found in persons, especially children, who have no *fever* at the moment of examination; but that is another matter. Such persons generally show other symptoms of the disease; and I know of no case which has been proved to

[1] See the articles by Copeland and Smith [1906], and Easton [1909], and page 214

have the parasites without having had any symptoms of the corresponding malady.

(2) *The parasites can always be found in a large proportion of malaria cases.* The amount of blood examined under the microscope is extremely small compared with the total amount of blood in a patient's body; so that if the parasites are not very numerous they may easily be overlooked, even by a skilled observer. Moreover, many cases come under observation only after the parasites have largely diminished in numbers; or do not remain under observation long enough for a thorough search for the parasites to be made. Hence we can scarcely expect to find the organisms without fail in every patient. But the experience of thirty years proves that the percentage of successes depends almost entirely on the care with which the search is made.

Since Laveran's discovery the parasites have been found by innumerable observers in large proportions of their cases. To mention a few—Laveran himself observed them in 432 out of 480 cases in Algeria [1891, p. 30]; W. S. Thayer and J. Hewetson in nearly all of 333 hospital in-patients in Baltimore [1895]; A. Billet in all of 395 cases in Algeria [1901]; W. Duggan in all of 400 cases in Sierra Leone [1897]. In Secunderabad, India, without attempting exhaustive search, I found the parasites in 69 out of 112 cases [1896]. Similar figures are given in most papers on the subject, and the matter has now become a commonplace of clinical medicine.

(3) *The patient's fever begins at the moment when the spores of the parasites are liberated.* This important fact was discovered by C. Golgi at Pavia, towards the end of 1885, and proves that the fever depends upon the parasites. In a masterly paper [1886] the author describes his study of forty cases of malaria, mostly quartan. In twenty-two of these he followed the development of the parasites in the peripheral blood *pari passu* with the progress of the malady; and he gives five of the cases in detail as examples of his finding.

The first classical case was that of a woman suffering from untreated simple quartan fever (attacks occurring every three days). Her blood was examined at 11.30 A.M. on the 2nd November, at the moment when her fever was expected to commence, and was found to contain a few full-grown parasites, and many other ripe ones full of spores. The attack commenced at noon while the blood was being examined. Next day there was no fever, and the blood contained only young parasites. The next day (4th November), there was still no fever, but the young parasites had now grown larger and occupied 3/4ths to 4/5ths of the containing haematids. On 5th November, three hours before the expected attack, the blood again contained nearly mature parasites, some with commencing spore formation. An hour before the attack these were increased in number. The attack then commenced at noon. Three hours later only a very few sporulating forms occurred; and five hours after the attack all of them had disappeared, being again replaced by numerous young parasites. On 6th and 7th November the same development of the parasites occurred as on the 3rd and 4th; and on 8th November there was a third attack of fever with parasites as on the 2nd and 5th. Quinine (1·5 grammes) was given on the 10th; and a fourth attack of fever occurred at 5.30 P.M. on 11th November.

The next case described by Golgi was one of "double quartan," that is, two sets of parasites reaching maturity on different days—one set giving severe attacks on 24th, 27th and 30th November, and the other mild attacks on the 23rd, 26th, 29th, and so on. The parasites of each set developed just as in the first case, independently of those of the other group. The third case was at first triple quartan; then, owing to the dying out of the different sets of parasites, double, and lastly single, quartan—the parasites always undergoing the same cycle of development. The fourth and fifth cases need not be described here.

These beautiful studies were quickly confirmed by W. Osler

[1886, 1887], by many Italian workers, and by Mannaberg. Such facts have also now become a commonplace of clinical medicine.

(4) *The different types of malarial fever are caused by different species of parasites.* Since the time of the ancients clinicians have been acquainted with three different types of fever, the quartan, the tertian, and the irregular or malignant type. This variation of type was equally surprising and inexplicable until Golgi, after elucidating quartan fever, commenced a study of tertian, and showed that it is produced by a parasite which is morphologically different from the quartan parasite, and reaches maturity every two, instead of every three, days. As in quartan, however, the patient's fever begins at the moment when the spores are liberated [1886, 1889]. Golgi also suggested that the third variety of fever is associated with a third variety of parasite—that which produces the well-known "crescents"; and this was proved to be the case by P. Canalis [1890] and Marchiafava and Celli [1890], who made a detailed study of the organism. Numerous confirmations followed, and the observations are now in general use for clinical diagnosis.

(5) *Both parasites and the fever may be reproduced in healthy persons by the inoculation of infected blood.* This has been done successfully in fifty-one cases, which will be described in the following section.

(6) *Both the parasites and the fever may be reproduced in healthy persons by the bite of infected mosquitos.* This has been done successfully in thirty-six cases, which will be described in section 17.

These facts are more than sufficient to prove that the parasites cause the disease. Additional but less precise arguments are:—

(7) *The severity of the illness depends roughly on the number of parasites present.* First suggested by Laveran and Golgi [1886], this theorem is now generally accepted, but rather on the grounds of common experience than on precise estimations.

(8) *Quinine reduces both the parasites and the fever.* This theorem, first suggested by Laveran, is now accepted everywhere, but still requires more precise study.

(9) *Certain secondary symptoms are caused by the accumulation of the parasites in certain organs.* As frequently proved in the case of the malignant parasites.

(10) *Both the parasites and the fever tend to disappear in a locality if the carrying Anophelines are greatly reduced in number.* As shown in many places, notably Ismailia.

15. Experimental Blood Inoculations. — These classical experiments, commenced independently of Laveran's discovery, and giving independent proof of a *contagium vivum* in malaria, are of fundamental importance.

The first experiments were those of A. Dochmann [1880]. One drop of the contents of vesicles of *Herpes labialis* of a boy suffering from quartan was inoculated subcutaneously into a healthy man on 8th February (?) 1880. The man had rigors and fever the same evening and on 11th February, and *malaise* on 14th February, no symptoms occurring in the intervals. Subsequently three men were similarly inoculated with herpetic serum from a girl with quotidian. This was followed by five days' quotidian in one of the men, and none in the others. Lastly, a girl was similarly inoculated on 12th April, and had fever on 14th and 16th April. There appears to have been no incubation period, and I agree with Laveran that the cases must be rejected. The admissible cases are now given and numbered for reference.

Case 1. Gerhardt [1884].—Source, quotidian fever; 1 c.cm. of blood taken at end of rigor and injected subcutaneously into B. S. on 11th August 1882. Subject had slight fluctuations of temperature 22nd to 26th August, and severe rigor and fever on 27th, 28th, 29th August. Quinine 2 grams on 29th; recovery after two days.

Case 2. *Ibid.*—Source, quotidian fever; blood (? quantity)

taken during attack, and inoculated into K. G. at 2.15 P.M. on 4th July 1883. Subject attacked at 6 P.M. on 10th July, and again at about 3 P.M. on 11th, 13th, 17th, 23rd, 25th, 28th, 29th, 30th (severe), 31st July. Quinine 2 grams daily, 1st to 6th August. Recovery followed.

Note.—The author appears not to have heard of Laveran's discovery; but his work was careful and good.

Case 3. Marchiafava and Celli [1885].—Healthy subject was inoculated subcutaneously with 0·5 c.cm. of blood taken from quotidian cases during attack on 18th July and 15th August (?) 1884. Again, intravenously with 0·5 c.cm. taken from a quotidian case during rigor on 21st August. Again, intravenously, with 1 c.cm. taken from a case of double tertian (? malignant) six hours before attack, on 26th August. Subject had strong rigor one hour after this last injection, and further attacks on 27th, 30th August, and 1st, 3rd September, when quinine was given. Isolated attacks occurred up to October.

Case 4. *Ibid.*—Subject, who had previously had malaria and was said to have been cured, was inoculated intravenously with 0·6 to 3·0 c.cm. of blood from various cases on 31st August, 4th and 6th September. Left hospital. Returned to hospital with fever on 10th September. Daily attacks like those of the source of blood inoculated on 6th September, the period of incubation being said to be seven days.

Case 5. *Ibid.*—Healthy subject inoculated with 1·0 c.cm. of blood from three cases, subcutaneously or intravenously on 6th, 9th and 13th September. Strong rigor on 20th September, and fever till 25th.

Note.—Confused experiments. Type of parasites not clearly seen (before Golgi's work). Mariotti and Ciarrochi, in whose clinics the work was done, also reported it.

Case 6. Gualdi and Antolisei [1889, No. 225]. Source reputed to be quartan; 3 c.cm. injected intravenously. Subject attacked ten days later with malignant parasites.

Case 7. *Ibid.*—Same source, dose and injection. Another subject. Attacked twelve days later, also with the malignant parasites.

Note.—In these two cases the quartan of the source seemed to have produced malignant in the subjects; but the authors stated later [1889, No. 264] that the source was subsequently found to contain malignant as well as quartan parasites, the former also being observed microscopically—so that the result of the experiments was not surprising.

Case 8. Antolisei and Angelini [1889, Nos. 226, 227].—Source, mild tertian; blood (? quantity) taken at beginning of fever; injection intravenous. Subject attacked with fever after eleven days; mild tertian parasites.

Case 9. *Ibid.*—Same source as previous case; 1·5 c.cm. of blood taken at the same time; injection intravenous. Subject attacked almost at same hour as previous case. The same parasites.

Case 10. Gualdi and Antolisei [1889, No. 264].—Source, quartan; 3 c.cm. of blood injected intravenously. Subject attacked after twelve days; quartan parasites.

Case 11. *Ibid* [1889, No. 274].—Source, malignant parasites with apyrexia and (?) crescents only; 2 c.cm. of blood, intravenous. Subject attacked with irregular fever on ninth day, and asexual malignant parasites on tenth day. Crescents on eighteenth day.

Case 12. Di Mattei [1891, No. 121]. Source, malignant; (?) quantity of blood injected intravenously. Subject, a case of quartan which had (?) recovered spontaneously. A few days

later small unpigmented asexual forms, and crescents twenty-five days after inoculation.

Case 13. *Ibid.*—Source, quartan; (?) quantity of blood, intravenous. Subject, a case containing crescents only. Subject attacked after fifteen days with quartan parasites, the crescents diminishing in number.

Note.—These two cases show that a second species of parasite can be added to a previous invasion. The disappearance of the crescents in *Case* 13 need not have been due to the new invasion.

Case 14. Calandruccio [? 1891].—Source, quartan; 1 c.cm. of blood, subcutaneously injected on 1st December 1890. Subject, the author himself, attacked with quartan on 18th December; relapses after two and three months.

Case 15. *Ibid.*—Source, a case showing (?) only crescents; 1·5 c.cm. subcutaneous. Subject, the author after recovery from previous infection. After fifteen days was attacked by severe rigor and fever, and then crescents after three days. No young asexual forms found. Several relapses.

Case 16. *Ibid.*—Source, quartan; 1 c.cm. of blood (?) subcutaneous. Subject attacked after twelve days with quartan.

Case 17. *Ibid.*—Source containing only crescents; 1·5 c.cm. (?) subcutaneous. Subject the same as in previous case, four months later. Crescents found in spleen on eighteenth day, and in peripheral blood on twentieth day. No asexual forms.

Note.—Author seems to have believed with Grassi and Feletti that the crescents were a species by themselves (*Laverania*).

Case 18. Bein [1891].—Source, mild tertian. Blood drawn by leeches, taken out by a syringe, and injected to the amount of 2 c.cm. in this and the three following cases. Subject attacked on twelfth day with mild tertian parasites.

Case 19. *Ibid* — Source, mild tertian. Same procedure. Subject attacked on twelfth day with mild tertian parasites.

Case 20. *Ibid.* — Source, mild tertian. Same procedure. Subject attacked on tenth day with mild tertian parasites.

Case 21. *Ibid.*—Source same as in last case. Same procedure. Subject attacked on tenth day with mild tertian parasites.

Note.—Four attempts failed. The injection was intravenous in one case, subcutaneous in the others. The type of *fever* was not always the same as that of the source, being sometimes simple and sometimes double tertian; but this is easily explicable. The same *parasite* was always found in the subject.

Case 22. Baccelli [1892].—Source, tertian; 3 c.cm. of blood injected intravenously. Subject attacked with double tertian parasites after six days.

Case 23. *Ibid.*—Source, quartan; 4 c.cm. of blood containing few parasites, intravenous. Subject attacked with single quartan after eleven days.

Case 24. Sacharoff [1894]. — Source, malignant. Blood drawn by leeches, which were kept on ice for four days; 1/4 c.cm. of this blood injected subcutaneously on 4th October (?) 1893. Subject, the author himself, had rigor and fever on 16th and 17th. Malignant parasites found on latter date.

Note.—This experiment was made during the course of study of the vitality of the parasites in leeches kept at freezing point. A second experiment, with blood kept thus for seven days, failed.

Case 25. Bignami and Bastianelli [1894.]—Source, malignant; 2 c.cm. taken at close of paroxysm; numerous parasites. Subject attacked after (?) three days with the same parasites.

Case 26. *Ibid.*—Source, malignant; 5 c.cm. of blood with moderate number of parasites, at close of paroxysm. Subject attacked after (?) four days with the same parasites.

Case 27. *Ibid.*—Source, malignant; 3/4ths c.cm. of blood with moderate number of parasites (? scanty). Subject attacked after five days with the same parasites.

Case 28. *Ibid.*—Source, malignant; 1/5th c.cm. with (?) numerous parasites. Subject attacked after four days with same parasites.

Case 29. Di Mattei [1895].—Source, quartan; 2 c.cm. subcutaneous, on 14th August (?) 1894. Subject attacked with simple quartan on 1st September (sixteen days).

Case 30. *Ibid.*—Same source and same injection on same date. Subject attacked with simple quartan on 25th August (eleven days).

Note.—Two other cases, injected subcutaneously with 0·5 and 1·0 c.cm. failed.

Case 31. *Ibid.*—Source, malignant; 2 c.cm. of blood from epistaxis injected subcutaneously in two places on 18th September. Subject attacked with malignant parasites on 3rd October. Fever on 3rd, 4th, 5th, 9th, 10th October. Crescents on 11th; quinine on 16th. Fever again on 17th; quinine; recovery.

Note.—In this paper author re-describes his earlier experiments [1891], and also describes eight negative attempts to transfer the malaria of birds from bird to bird or bird to man, and four negative attempts to infect birds from men.

Case 32. Bignami [1898].—Source, malignant; less than one drop of blood, subcutaneous. Subject attacked after six days with the same parasites.

Case 33. *Ibid.*—Similar to the above. Subject attacked after ten days with the same parasites.

Case 34. Mannaberg [1905].—Source, benign tertian; 0·2 c.cm. of centrifugated blood taken during paroxysm; sediment injected subcutaneously. Subject attacked after twenty-one days with the same parasites.

Case 35.—Celli and Santori [1897]. Source, quartan; 4 c.cm., subcutaneous. The same parasites in subject after twenty-five days.

Case 36. *Ibid.*—Source, quartan; 4 c.cm., subcutaneous. The same parasities after twenty-five days.

Case 37. *Ibid.*—Source, quartan; 4 c.cm., subcutaneous. The same parasites after twenty-five days.

Case 38. *Ibid.*—Source, malignant; 1·5 c.cm., subcutaneous. The same parasites after thirty days.

Case 39. *Ibid.*—Source, malignant; 1·5 c.cm., subcutaneous. The same parasites after six days.

Case 40. *Ibid.*—Source, malignant; 1·5 c.cm., subcutaneous. The same parasites after seventeen days.

Note.—In all these six last cases the blood was mixed with sera of horse, buffalo or cattle before being injected, in the hope of immunising the subject.

Case 41.—Elting [1899]. Source, benign tertian; 3 c.cm. of blood containing half-grown parasites injected intravenously on 7th August (?) 1898 at (?) Baltimore. Subject had a slight rise of temperature next day and sharp rise to 39·9° C. on 10th. Benign tertian parasites next day.

Case 42. *Ibid.*—Same source; 2 c.cm. taken and injected the same day. Fever on fifth day; benign tertian parasites on the previous day.

Case 43. *Ibid.*—Source, malignant, with numerous asexual forms and a few sexual ones. Blood injected (?) 1·5. to 4·0 c.cm.

(?) intravenously on 28th August (?) 1898. Fever night of 30th to 31st; malignant parasites on 31st; crescents on 6th September.

Case 44. *Ibid.*—Same source, method and date. Asexual malignant parasites found in subject 1st September with commencing fever. High fever on 3rd, and crescents on 8th.

Case 45. *Ibid.*—Same source and method. Inoculation of subject 29th August, followed by a short rise of temperature in ten hours, after which there was no fever till 5th September. Asexual malignant parasites on previous day (4th), and crescents on 11th.

Case 46. *Ibid.*—Source, malignant—*Case* 43 above. Blood (?) quantity taken a few days after appearance of asexual forms, and (? how) injected on 1st September. Asexual malignant parasites found on third day after injection, and thirty-two hours before first rise of temperature. Crescents on ninth day.

Case 47. *Ibid.*—Same source, method and date. Fever on 4th September, and asexual malignant parasites on same day. Crescents on 11th.

Case 48. *Ibid.* — Source, malignant — *Case* 46. Blood contained numerous asexual forms, and 1·5 c.cm. injected intravenously on 9th September. Asexual malignant parasites on 13th, fever on 14th, crescents on 20th.

Case 49. *Ibid.*—Two sources—malignant and benign tertian. Two or three drops of blood containing only malignant parasites injected subcutaneously (?) date. Next day 4 c.cm. from benign tertian case injected intravenously. Short rise of temperature two hours after second injection; then normal for five days. Fever seventh day after first injection, and malignant parasites next day. Benign tertian parasites ten days after second injection; both flourished together. Crescents seventeen days after first injection.

Case 50. *Ibid.*—Two sources—benign tertian and malignant

later. Blood from former, 4 c.cm. containing numerous parasites, injected subcutaneously. Slight rise of temperature six hours later. Marked fever on seventh day, and benign tertian parasites on tenth day.

On the third day after first rise of temperature (? ten days after first inoculation) the subject was inoculated with (? quantity of) blood containing numerous asexual malignant parasites — intravenously. These were recovered in blood of subject four days later, together with numerous tertian parasites. Fever suddenly ceased eighteen days after first injection ; then seven days apyrexia with only one or two tertian parasites each day. But twenty-six days after first injection, quotidian fever with both parasites began. Quinine and cure.

Case 51. Rosenau, Parker, Francis and Beyer [1904].—At Vera Cruz. Source, double benign tertian ("heavy" infection); 2 c.cm of blood during rigor, mixed with equal part of salt solution, injected intravenously into subject at 2 p.m. on 6th November 1903. Slight reaction within an hour. Typical fever and parasites at 7 a.m. on 10th November, double infection (section 19 (4)).

This closes the list of *positive* experiments, but four important *negative* ones with blood containing (?) only sexual parasites remain to be recorded.

Negative Case 1. Thayer [1898, p. 75].—Source, a patient, convalescent from first attack, had had quinine for four days, during which none but crescentic and ovoid forms were found in the peripheral circulation. A hypodermic syringe full of his blood was injected into median basilic vein of subject in August (? year). The latter was carefully observed for five weeks, but never showed fever or parasites.

Negative Case 2. Elting [1899].—Source, malignant, with crescents and asexual forms, treated for eight days with four hourly doses of 5 grains (0·325 grams) of quinine, until only the crescents were left. Subject, injected intravenously with

3 to 3·5 c.cm. of blood on (?) date, never showed fever or parasites.

Negative Case 3. *Ibid.*—Same source, injection and date. Subject showed neither fever nor parasites.

Negative Case 4. *Ibid.*—Same source, injection and date. Subject had slight transient rises of temperature two and six days after inoculation. No characteristic fever or parasites.

Note.—These four cases give strong reason for supposing that the sexual forms (crescents) are not in any way concerned in the reproduction of the parasites in the blood of the patient who contains them (section 20 (4)).

I am much indebted to Professor A. Celli for obtaining for me from Professor Giulio Galli an account of two interesting experiments performed by the latter, showing that malarial blood may be inoculated without result into subjects who are taking quinine.

Negative Case 5. G. Galli [?].—Source, benign tertian, many parasites; 0·5 c.cm. of blood was injected subcutaneously into subject, a healthy woman who had been given 0·4 grams of "state" quinine daily for five days. The same dose was continued on 1st, 2nd, 5th, 6th, 7th, 12th, 13th, 14th day after inoculation, and then stopped. Subject remained in hospital for forty-two days after inoculations, but showed no sign of malarial infection.

Negative Case 6. *Ibid.*—Same source, amount of blood and method of inoculation. Subject, a healthy woman, was given 0·4 grams of quinine daily two days before inoculation, and six days after. She remained in hospital thirty-two days after inoculation, but showed no sign of malarial infection.[1]

Remarks.—I have verified most of these cases from the original literature; but for a few of them the literature has not been obtainable. I have also omitted a few cases regarding which I can obtain no references.

[1] See also section 65 (5).

16. The Parasites develop in Certain Mosquitos. — I commenced the study of the mosquito-theory of malaria in Secunderabad, India, in April 1895. Numbers of mosquitos of the genera *Culex* and *Stegomyia* were allowed to feed on patients whose blood contained the sexual forms of the parasites, and were afterwards dissected in the hope of finding in them some developmental stage of the organisms. This investigation was continued without success until August 1897. Many hundreds of insects of various species (undetermined) of these genera were thoroughly searched, and many hundreds more were examined less completely — strong evidence in favour of the view that the parasites do not develop in these kinds of gnats.

On the 20th and 21st August 1897, I found the zygotes of the malignant parasite in two Anophelines (species undetermined) bred from the larvae and fed on a case of crescents. In September, I found them again under the same conditions in a mosquito of another species of Anopheline [December 1897 and February 1898.]

About the same time MacCallum and Opie demonstrated the sexual nature of the crescents [1897].

On the 20th March 1898 I found the zygotes of one of the malaria parasites of birds, *P. danilevskyi* Grassi and Feletti 1890 (*Proteosoma* Labbé), in *Culex fatigans*, and worked out the life-cycle of these parasites in the insects [1898]. It was however of fundamental importance to prove that the bodies found in the mosquitos were really descended from the parasites found in the birds; and the following proofs were obtained :—

(1) The bodies in the mosquitos contained the characteristic plasmodin (pigment) of the parasites in the birds.

(2) Their growth and development were observed in detail in the insects.

(3) Out of 249 mosquitos fed on birds with no parasites or other parasites, not one was found to contain the

bodies referred to; but, out of 245 mosquitos fed on birds showing *P. danilevskyi*, 178 or 72% contained them.

(4) Out of 10 mosquitos fed on a sparrow without parasites, none contained the zygotes. In 10 mosquitos, fed at the same time on a sparrow with moderate parasites (1/1000[1] haematids), 292 zygotes were counted. In 10 mosquitos fed at the same time in a sparrow with many parasites (1/50 haematids) 1,009 zygotes were counted.

(5) In July to August I succeeded in infecting 22 out of 28 healthy sparrows, and some other birds, by the bites of infected mosquitos [1898, 1905].

This work was speedily confirmed by R. Koch [1899], by C. W. Daniels [1899], and, subsequently, by many others.

I completed my work on human malaria in 1899 in Sierra Leone [1899, 1900]; but in the meantime Bignami, Bastianelli and Grassi had already observed that the human parasites have the same life-history [1898-1899.] Since then the life-cycle of the parasites in mosquitos has been studied by many workers (section 6).

17. Experimental Mosquito Inoculations. — We should begin by referring briefly to the early studies which showed that malaria cannot be communicated by marsh water. After the efforts of Salisbury in 1866, Balestra in 1869, Safford and Bartlett, Archer, Bargellini, Klebs and Tommasi-Crudeli — well summarised in the paper of the last named [1879]—to incriminate various organisms as the cause of the disease, several Italians carried out good researches on the effect of drinking water. Celli failed in infecting six people by water brought from the Pontine Marshes [1886]; and Brancaleone and Marino performed similar experiments. The work of Agenore Zeri [1890] was very complete. He gave water from

[1] Very roughly computed.

the Pontine marshes and elsewhere to nine persons to drink in doses between 3 and 1·5 litres a day for five to twenty days each. To sixteen others he administered the water in the form of inhaled spray; and to five others by rectal injection. All the experiments were negative. Yet they should have succeeded if we suppose that the plasmodia live in such water and infect either by the respiratory or digestive passages—unless we assume that they died in the water during transit to Rome, where the experiments were conducted.

In 1895-1896 I made twenty-two experiments with a view to infecting healthy persons with drinking water in which mosquitos had been allowed to lay their eggs and die [1896]. The first case was, by a coincidence, attacked with fever; but as all the rest proved practically negative, I decided that no reliable evidence had been obtained one way or the other.

A. Bignami, in a paper discussing Manson's hypothesis [1896], stated that he had failed some time previously in causing infection by the bites of mosquitos brought from malarious places. He believed with King that the insects bring the poison from the marsh, and I presume that his mosquitos were therefore collected at random. This hypothesis was not in accordance with parasitological teaching.

The first correct experiments on mosquito-inoculation were performed by myself in August 1896. At that time I began to abandon part of Manson's hypothesis according to which the insects take the parasites from man and deposit them in water—I began to think that the insects take the parasites from man and also inoculate them into man. A number of several kinds of *Culex* and *Stegomyia* were therefore allowed to feed on various cases of malaria—one of which contained all the three species of parasites—and were then fed, after being kept for several days and on several occasions, upon Mr Appia, Assistant-Surgeon to my hospital in Bangalore, India. The result was entirely negative, the mosquitos being of the

wrong kinds, and the interval between the feedings too short [30th October 1896].

The first successful mosquito inoculations were those of birds, Calcutta, June to August 1898. Four sparrows and a weaver-bird, which had frequently been found to contain no parasites, and had been often used for controls in my laboratory for that reason, were bitten towards the end of June, on several nights in succession, by numbers of *Culex fatigans* heavily infected with *P. danilevskyi.* On 9th July they were all found to contain "swarms of the parasites." The experiment was next repeated on many more birds under proper conditions, with the following results:—

(1). Out of twenty-eight healthy sparrows twenty-two or 79% were infected in this manner.

(2). One of the six sparrows that escaped the first experiments was infected on a second trial (the remainder died).

(3). Out of two crows and four weaver-birds, free from *P. danilevskyi*, one of the crows and all the others became infected.

(4). Out of five sparrows, originally containing a very few *P. danilevskyi*, four showed a much more copious infection after the experiments.

The experiments were quite decisive. At that time in Calcutta I found these parasites only in fifteen out of one hundred and eleven wild sparrows, and then only in small numbers in the blood. But, as I used large numbers of heavily-infected mosquitos, the produced infections were extremely copious, as many as ten to sixty parasites being counted in each field of the microscope (oil-immersion lens). The incubation period was always four to seven days. This success was reported in England by telegram, and was published by Manson at the end of July 1898.

The following experiments have subsequently been performed on men.

Case 1. Bignami [November 1898].—Some months later Bignami succeeded in infecting a man by following my methods. A number of mosquitos caught in infected houses at Maccarese were fed on a malaria-free inmate of the Santo Spirito Hospital, situated in a part of Rome where there is no malaria. They were fed on him nightly from 26th September to the end of October 1898, and belonged to *Anopheles maculipennis*, and two species of Culicines. The subject was attacked with slight fever on 31st October, and severe fever the next two days; quinine being given on 3rd November. In spite of careful search, the parasites (malignant) were not found until 3rd November, about forty hours after the first marked rise of temperature. Several relapses occurred later.

Case 2. Bastianelli, Bignami and Grassi [1898-1899].—*Anopheles maculipennis* caught at Maccarese were fed from 13th November to 2nd December 1908, on a healthy subject in the same hospital. Fever commenced on 3rd December and continued daily until 9th; quinine on the 7th. Mild tertian parasites found on first day of fever. Parasites found also in the mosquitos.

Note.—This fortunate experiment revealed that the tertian parasites also are carried by *A. maculipennis.* Gametids appeared on 6th December.

Case 3. *Ibid.*—Seven *A. maculipennis* caught in an infected house at Tre Fontane were fed, each one once, on 10th, 11th, 13th December 1898 on a subject in the same hospital. The protospores were found in the salivary glands of two out of three of these insects examined. Subject attacked with fever on 29th December, the mild tertian parasites being found next day—two sets. Gametids on the 31st.

Case 4. *Ibid.*—Three *A. maculipennis* were fed on a case of crescents between 10th to 18th December 1898, were incubated at 30° C. for two days, and were then re-fed on a

healthy subject on 2nd and 5th January 1899. All three mosquitos were found to be infected, and two of them had protospores in their salivary glands. Subject was attacked with fever from 14th to 18th January. Quinine on 16th. Malignant parasites found on 15th and disappeared under quinine on 17th. No crescents found.

Case 5. P. Manson [1900].—Ten *A. maculipennis* were fed on a case of double benign tertian in Rome on 17th, 20th, 23rd August 1900, and bit P. Thorburn Manson in London on 29th and 31st August, and 2nd and 4th September. Also thirty-five of same species were fed on a simple benign tertian case in Rome on 6th and 7th September, and on the same healthy subject in London on 10th and 12th September. Fever began on 13th September, and continued on 14th, 15th, 16th and 17th. Benign tertian parasites found on 16th for the first time. Quinine on 17th. Relapse nine months later.

Case 6. Rees [October 1900].—A second subject, Mr Warren, was bitten by the second batch of mosquitos fed on previous subject at about the same time (not given). Fever commenced on 28th September after about fourteen days' incubation, and seemed to have continued for some time. Parasites in phagocytes on 30th. Benign tertian parasites on 2nd October and subsequently. Quinine on 3rd. Author says that this case showed more parasites than the previous one, as he was bitten by more mosquitos.

Case 7. Fearnside [1901].—The following series of seven cases were obtained at Rajamundri, India, in 1900-1901. Unfortunately, the Anophelines were not identified, but the author states that only one species was used. Source, benign tertian, copious, sporulating on 17th December, when the Anophelines were fed. Insects were re-fed on the author himself on 20th December and 1st and 8th January 1901. Tertian fever began on the 14th. Benign tertian parasites found on 18th. No quinine.

Note.—Author had suffered from malignant malaria with haemoglobinuria in 1891; but that, apparently, did not affect the present experiment.

Case 8. Fearnside [1901].—Source the same, and Anophelines fed on same date. Subject (an Indian) bitten on 28th December. Tertian fever on 18th January. Benign tertian parasites on 21st.

Case 9. *Ibid.* — Sources, two cases of benign tertian. Anophelines fed 12th and 13th December. Bit subject (Indian) on 27th. Benign tertian on 11th January. Benign tertian parasites on 13th. In notes, author says that only one Anopheline inoculated the subject.

Case 10. *Ibid.*—The same source. Anophelines infected 12th December and bit subject (Indian) on 26th. Tertian fever on 20th January, and tertian parasites on 25th. Apparently the same (one) mosquito used as in previous case.

Case 11. *Ibid.*—Source, double infection of malignant and of benign tertian. Anophelines infected 16th December, and bit subject (Indian) on 28th. Fever on 9th January, and malignant parasites on 11th; doubtful tertian parasites on 12th.

Case 12. *Ibid.*—Same source. Anophelines fed on same date and bit subject (Indian) on 28th December. Fever, 13th January. Malignant and benign tertian parasites on 14th.

Note.—Of two other cases experimented with by Fearnside one appears to have failed, and one to have contracted fever after fifteen days, the finding of the parasites not being noted. Author states that protospores were found in all the Anophelines used.

Case 13. Buchanan [1903].—Experiments done at Nagpur, India. Source, malignant. Anophelines (species not given) fed 25th December 1901 to 8th January 1902, and bit subject

10th to 17th January. Fever 27th January, and malignant parasites.

Case 14. Buchanan [1903].—Source, malignant. Anophelines fed 13th to 23rd January 1902, and bit subject 24th to 27th January. Fever 12th February, and malignant parasites.

Case 15. *Ibid.*—Source, malignant. Anophelines fed 24th January to 4th February 1902, and bit subject 7th to 19th February. Fever 19th February, and malignant parasites.

Case 16. *Ibid.*—Source, malignant. Anophelines fed 27th January to 4th February 1902, and bit subject 4th to 20th February. Fever 23rd February, and malignant parasites.

Case 17. *Ibid.*—Source, malignant. Anophelines fed 29th January to 11th February 1902, and bit subject 12th to 23rd February. Fever 3rd March, and malignant parasites.

Note.—Three attempts to infect from malignant sources failed. In two other cases, however, there was no fever after the mosquito inoculations, but crescents were found after twelve and eight days. These are not accepted here.

Case 18. *Ibid.*—Source, quartan. Anophelines (? species) fed 30th January to 11th February 1902; and bit subject from 14th to 27th February. Fever 25th, and malignant parasites (*not* those *found* in source).

Note. — Four other cases from quartan sources failed. Anophelines not identified.

Case 19. *Ibid.*—Source, benign tertian. Anophelines fed 8th to 20th January 1902, and bit subject 21st to 25th January. Fever 4th February, and benign tertian parasites.

Case 20. *Ibid.*—Source, benign tertian. Anophelines fed 15th to 25th January 1902, and bit subject 28th January to 10th February. Fever 15th February, and malignant parasites (*not* those *found* in source).

Case 21. Buchanan [1903].—Source, benign tertian. Anophelines fed 1st to 13th February 1902, and bit subject 15th to 25th February. Fever 23rd February, and benign tertian parasites.

Case 22. *Ibid.*—Source, benign tertian. Anophelines fed 2nd to 14th February 1902, and bit subject 17th to 22nd February. Fever 1st March, and benign tertian parasites.

> *Note.*—Five other attempts to infect from tertian sources failed.

Case 23. Schüffner [1902].—Experiments in Sumatra with two species of Anophelines, apparently *Cellia kochii* Dönitz, and a *Myzomyia.* Source, benign tertian. Anophelines fed (?) 15th July (?) 1902, and bit subject, the author himself, on two days (?) 25th July. Fever, double tertian, 11th August, and benign tertian parasites.

Case 24. *Ibid.*—Same source, and Anophelines fed same date, and bit another subject on same date. Fever on same date, single tertian; and tertian parasites.

Case 25. *Ibid.*—Source, malignant. Infected Anophelines bit subject (Chinaman) on 20th, 21st August (?) 1902. Fever 5th September, and malignant parasites 7th.

Case 26. Jancsó [1905]. — A long series of admirable experiments on the effect of temperature on the development of the parasites in *Anopheles maculipennis*; carried out at Kolozsvar, Hungary. Source, benign tertian. Six *Anopheles* fed 23rd September (?) 1904, and kept at 21° C. for twenty-three days. Subject bitten 16th October, had fever on 15th day Benign tertian parasites on 16th day.

Case 27. *Ibid.*—Source, malignant. Fifty-two *Anopheles* fed 26th October (?) 1904, kept at 30° C., and bit subject 3rd to 13th November. Fever 15th; malignant parasites 16th.

Case 28. *Ibid.*—Same source. *Anopheles* fed on 29th

October and kept at 30° C. Subject bitten 6th to 8th November by more than twelve. Fever 16th, and malignant parasites on 18th.

Case 29. Jancsó [1905].—Same source, and mosquitos. Subject bitten by six on 16th November. Fever 22nd, and malignant parasites 26th.

Case 30. *Ibid.*—Source, malignant. *Anopheles* fed 15th November and kept at 22-24° C. Subject bitten by seven on 30th. After ten days, fever and malignant parasites.

Case 31. *Ibid.*—Same source and mosquitos. Subject bitten by one *Anopheles* on 3rd December. After fourteen days, crescents in blood.

Case 32. *Ibid.*—Same source and mosquitos. Subject bitten on 6th December by two *Anopheles.* Quinine 1-2 grams on 12th and 13th. Fever and malignant parasites on 18th.

Case 33. *Ibid.*—Source, malignant. *Anopheles* fed 24th September at 30° C., and afterwards kept at 20° C. Subject bitten on 21st October by one *Anopheles*, strongly infected. Quinine 1 gram on 28th and 29th. Fever on 4th November and crescents on 6th.

Case 34. *Ibid.*—Source malignant. *Anopheles* fed 24th September and 1st October at 30° C., and then kept at 15-17° C. Subject bitten 7th November by two of these with glands infected. Fever and crescents on 18th.

Case 35. *Ibid.*—Same source. *Anopheles* fed 25th September and 1st October and kept at varying temperatures from 8-30° C. Subject bitten by two of these (both infected) on 13th and 18th October. Fever 24th and crescents 25th.

All these cases have been verified by me in the original literature; but there are probably some other experiments which have been overlooked.[1]

[1] See section 65 (5).

CHAPTER IV

THE PARASITIC INVASION IN MAN

18. The Onset of the Invasion.—It does not lie within the province of this book to give a full description of the pathology and symptoms of malarial fever—a subject which is dealt with in many works. But before proceeding to our proper theme, we shall do well to examine a certain number of questions, especially some upon which little stress has been laid in the publications referred to. We should begin by attempting to obtain a clear picture of the onset and progress of the parasitic invasion in man.

Our first care should be to consider the *number* of organisms engaged in the invasion—a subject which has been much neglected. For example, in the eighty-six cases of successful experimental inoculations of men, I cannot find a single one in which correct estimates of the number of parasites, in the source, the carrier, or the subject, have been even attempted. Certainly, these experiments prove the main theorem, that the parasites cause the disease; but they might easily have been used to obtain many valuable pathological data in addition. We are thus forced (at present) to rely largely upon calculation for our figures.

(1). *The number of parasites in the mosquito.*—The number of parasites which enter a mosquito when it feeds on a patient depends (*a*) on the amount of blood sucked up by it, and (*b*) on the number of parasites in that blood. The insects feed during variable periods—for a few seconds if disturbed, or perhaps

during the whole night upon a sleeping person. While feeding they void, every ten seconds or so, some of the fluid part of their meal; so that during a whole night they may possibly consume much more blood than one stomach-full. A mosquito fully gorged in this manner may (perhaps) consume several cubic millimetres of blood. It would be easy to settle this point by allowing a number of mosquitos to feed during 1, 2, 3, 4 . . . minutes, and then killing and weighing them and their dejecta. For example, as R. Newstead has shown ("Reports Liv. Sch. Trop. Medicine, Liverpool," 1905, vol. xvii. p. 25), the tick *Ornithodorus moubata* weighs 0·027 grams before feeding and 0·260 grams after feeding. D. Thomson and myself find, in researches now being conducted in my clinic in Liverpool, that 6-7000 crescents per c.mm. of blood is not an exceptionally high number. Supposing that only half of these are females, it is still very unlikely that so many could ever develop, after being fertilised, in one insect—a large or very large proportion probably perish from phagocytosis and other causes in the stomach contents. The largest number of developing zygotes found by me in one insect (*C. fatigans* gorged on blood with *P. danilevskyi*) was 445. Ten of these mosquitos fed all night on a bird with moderate parasites (1/1000 haematids) were found to contain an average of 29 zygotes each; and ten of them fed on a bird with many parasites (1/50 haematids) contained an average of 100 zygotes each [May 1898]. But I do not know what proportion of the parasites in these birds were sexual forms.[1] The number of zygotes found in Anophelines naturally fed on human blood does not very often, I think, exceed fifty—but this is probably due to the manner of feeding. The number of ingested sexual parasites which reach maturity and develop protospores depends (*a*) on the susceptibility of the insect, (*b*) on the temperature, and (*c*) possibly many other conditions. Jancsó [1905] finds that the zygotes develop best at 24-30° C.,

[1] See reference to paper by S. T. Darling in section 65.

temperatures above and below these limits retarding the process (*A. maculipennis*); and that they die if the mosquito is kept constantly below 16° C. after feeding. On the other hand, they often continue to grow if the carrier is subjected merely to an intermittent low temperature. Regarding the susceptibility of various species of mosquitos much work remains to be done.

(2). *The number of protospores in the salivary glands.*—I may have overlooked references, but do not know that the average number of protospores in matured zygotes has ever been exactly estimated. I should give the number (merely as an impression) at about a thousand. Not all of these effect an entry into the salivary glands. Here, again, there seem to be no exact counts. I fancy that more than 10,000 of them will seldom be found in the glands of a single Anopheline, while often there may perhaps be only a few hundreds at a time (subject to correction).

(3). *How many protospores enter the human blood?*—This must depend (*a*) upon the number of spores in the biting insect's salivary gland, and (*b*) upon the number of times it is allowed to bite its victim. I think that mosquitos inject their poison before commencing to suck. If this is the case, an insect which bites a person several times (as, for instance, when he is asleep) is likely to inoculate many more protospores than one which succeeds in biting only once. In the former case several thousand spores may perhaps be introduced; in the latter case perhaps only a few.

But not all of the spores which have entered are likely to live. Probably many perish by falling outside the blood stream or by becoming a prey to phagocytes.

(4). *Further development of the protospores.*—F. Schaudinn observed that if protospores are taken from a mosquito's salivary gland and are mixed with blood under the microscope, many of them may actually be watched entering the haematids, where they become the young intracorpuscular parasites familiar to students. These now begin to grow and to develop a second

generation of spores, which should be called *deuterospores*. The latter attack fresh haematids, within which they grow, and develop a third generation of spores, which we may call *tritospores*; and so on indefinitely.

(5). *The number of spores produced by each species of parasite.*—The parasites of malaria are described in detail in the text-books. Since the time of Golgi, all observers admit that they belong to three types at least, each of which differs morphologically, and markedly so, from the others. I assume, for the sufficient reasons frequently given, that these three types are three different species. The number of spores produced by each species is variable, and different authors give different figures (probably the number has never been accurately estimated). I adopt the following names and figures for the present:—

Plasmodium malariae Laveran, 1881. The Quartan Parasite; produces say 6-12 spores every three days.

Plasmodium vivax Grassi and Feletti, 1890. The Benign Tertian Parasite; produces say 15-20 spores every two days.

Plasmodium falciparum Welch, 1897. The Malignant Parasite; produces say 6-20 or more spores every two days.

Some authors consider that there are two if not three varieties (or ? species) of malignant parasites. I am inclined to agree with them, but have not yet satisfied myself sufficiently on the point to admit it in my classification.

(6). *The onset of the invasion.*—Let us suppose that a mosquito has inoculated several thousand protospores, but that only one thousand of these have succeeded in entering the haematids. After two or three days, according to the species of parasite, each will produce a variable number of deutero-spores. But, probably, not all of these will succeed in infecting fresh haematids; many may be devoured by phagocytes, or be destroyed by other agencies, while passing from one haematid to another. And the same thing is likely to happen with every successive generation of spores. Hence only a proportion of

the spores actually produced are likely to enter fresh corpuscles.

Suppose that 1,000 protospores of *P. vivax* have entered haematids, and that, on the average, only 10 out of the 15-20 spores actually produced in the successive generations succeed in entering fresh haematids. The parasites should then multiply as follows:—

No. of days	0	2	4	6
No. of parasites	1,000	10,000	100,000	1,000,000

No. of days	8	10	12
No. of parasites	10,000,000	100,000,000	1,000,000,000

and so on.

In the case of *P. malariae* the multiplication should be slower, if the number of spores given above is correct. But in that of *P. falciparum* the data are too uncertain for calculation.

(7). *The number of haematids in an average man.*—According to accepted standards we have—

(*a*) 1 c.mm. of blood contains 5,000,000 haematids.

(*b*) The specific gravity of blood (male) is about 1057·5.

(*c*) The total amount of blood weighs about 4·9/100 of the total weight of the body.[1]

From these data we calculate that 3,000,000 c.mm. of blood weigh 3·17 kilograms, and will be contained in a man weighing 64·74 kilograms (142 lbs. English, or about 10 stone), and that these 3,000,000 c.mm will contain 15,000,000,000,000 haematids.

This may then be taken as the normal number of haematids in an average healthy man of about 64 kilograms, or 10 stone, in weight. The reader should endeavour to form some tangible idea of this enormous number. If he were to try to count it at the rate of 100 a minute day and night without cessation, he would have to spend 285,000 years over the task!

[1] The ratio of blood to body weight was formerly estimated at about 1/13, but the estimate has recently been reduced in consequence of better methods of measurement.

(8). *The lowest number of parasites required to produce the first illness.*—We can scarcely imagine that one protospore, or even many thousands of these minute bodies, can produce any marked symptoms in the patient; and, in fact, we learn from the fundamental experiments that as a rule no symptoms appear for some days after the moment of inoculation—this period being commonly called the *incubation period.* But as the parasites increase at every generation, a time must come when the number of them will suffice to cause illness. Can we determine this number? No serious attempt has yet been made to do so, or, indeed, to find any exact correlation between the number of parasites and the amount of sickness. There are many difficulties in the way. It is not always easy to estimate the number of parasites, and still less to know when precisely the illness commences. In most of the experimental inoculations, some illness, or even sharp fever, has occurred before any parasites have been found by the microscope. On the other hand, Elting has been able to find the parasites in some of his cases before the onset of the fever. The question is probably determined partly by the skill and patience of the observer, partly by the susceptibility of the patient, and partly by the "virulence" of the parasites. A laborious search may reveal the parasites when they are comparatively few in number; and on the other hand, a patient who has never been previously infected will probably begin to suffer earlier during the course of the invasion than one who is partially "immune." But for a broad general rule we may, I think, accept the principle (pending more exact researches) that if we cannot find the parasites after careful search, their number is not usually sufficient to produce fever. Hence I calculate that they will not generally be numerous enough to cause illness unless there is at least one parasite to 100,000 haematids; that is, 50 parasites in 1 c.mm. of blood; or 150,000,000 in a man of 10 stone (64 kilograms) in weight.

(9). *The time required to examine blood microscopically.*—It

is very necessary to have clear ideas upon this point. Suppose that the diameter of a "field" seen by an oil-immersion Objective and a No. 2 Ocular measures 0·165 mm., or nearly 1/6th of a millimetre. Then by moving the specimen across this field of vision, searching it as we move it, we shall cover, after traversing sixty times the diameter, a strip nearly 1·0 centimetre long, and 0·0165 cm. broad. Examining strip after strip in the same way, when we have examined sixty strips we shall have covered nearly one square centimetre of area.

The time required for this will depend upon the care with which we must examine the successive fields as they pass under the eye. If we can search at the rate of twenty fields a minute, we can search a whole strip in three minutes and a whole square centimetre in three hours. If the object is large and easily visible we shall be able to move the specimen faster than this; if it is small and delicate, the observer must be fairly expert to search so quickly.

Now suppose that we must examine a thin film of liquid blood spread under a coverglass. If the average depth of the film is only 0·00025 mm. ($2{\cdot}5\mu$ or about 1/3 the diameter of haematid), then 1 c.mm. of the blood will be spread out under 4 sq. cm. of area — that is, over the whole area occupied by a square coverglass of 2 cm. side. To do this will require twelve hours' continuous work. If the average depth of the film is $3{\cdot}33\mu$ the c.mm. of blood will cover a space of 3 sq. cm., requiring nine hours' search; but in this case we shall be more liable to overlook small objects, though we shall more easily find large ones.

With an average depth of $2{\cdot}5\mu$, 1 sq. cm. of film will normally contain 1,250,000 haematids. One-sixtieth of this, that is, one strip as defined above, will contain 20,833 haematids; so that, if we are fairly expert, we should be able to search 6,944, or say 7,000 haematids a minute. With an average depth of $3{\cdot}33\mu$, 1 sq. cm. of film will contain 1,666,666

haematids, which can be searched at the rate of 9,259 haematids a minute. In the former case a single circular field of the microscope, 165μ in diameter (area 0·0209 sq. mm.), will contain an average of about 261 haematids. In the latter case it will contain about 345 haematids on the average. There are, of course, more than 3,600 squares of 165μ side in a square centimetre; and therefore four and three times this number of squares must be examined to search 1 c.mm. of blood in films of $2·5\mu$ and $3·33\mu$ in depth respectively. If we could examine the squares at a rate so fast as one a second, three to four hours would still be required to search 1 c.mm. completely. To search the whole of the 3,000,000 c.mm. of blood contained in a man of about 10 stone in weight would therefore take, at the quickest rate, more than 1,027 years.

I have just estimated roughly that the parasites will probably be numerous enough to cause fever if they number 1/100,000 haematids. If it requires twelve hours to search 1 c.mm. of blood containing 5,000,000 haematids, 100,000 haematids can be searched in a little less than fifteen minutes; so that if the parasites are so few as this we can expect to find them at the rate of about one every quarter of an hour. But chance intervenes here: if we are lucky we may find the first parasite almost at once; if we are unlucky we may have to search several hundreds of thousands of haematids before finding an infected one. There is always the danger of overlooking a plasmodium even when it should have been seen.

In a dry stained film the blood is spread out over a wider area, so that there are only about 150-200 haematids in a circular field of 165μ. I think therefore that the larger pigmented parasites are less quickly found in such than in liquid films; but on the other hand, the smaller plasmodia, being characteristically coloured, are detected with much greater certainty—as Marchoux showed [1897]. On the whole, I think that the two methods are about equal in diagnostic value.

In my "thick-film" process [1903], 1 c.mm. of blood

occupies only about 1/5th of a sq. cm. of area, or less, so that there should be twenty to thirty times more haematids and parasites per field. But the latter require more skill for detection (section 65).

Such calculations demonstrate the absurdity of supposing that there are no plasmodia present in a person because we fail in finding one after a few minutes' search. As a matter of fact, even if as many as 150,000,000 plasmodia are present in an average man, the chances are that ten to fifteen minutes' search will be required for each plasmodium found; while if we are careless or unfortunate we may have to look much longer.

(10). *The period of incubation.*—To resume our study of the invasion—we saw in subsection 6 that, in the case of *P. vivax*, 1,000 protospores should produce 100,000,000 parasites in ten days and 1,000,000,000 in twelve days. The former number would probably be insufficient to produce fever in the patient, and the latter would be more than sufficient. Hence the illness would probably begin on the twelfth day after inoculation.

It is usually thought that the incubation period must depend exactly upon the number of organisms injected either by the mosquito or by the experimenter; but this is not always correct. The proliferation of *P. vivax* at the rate of ten spores at each generation every second day, starting with various initial numbers, should be as follows:—

No. of days	0	2	8	10
No. of parasites	1,000	10,000	10,000,000	100,000,000
	2,000	20,000	20,000,000	200,000,000
	3,000	30,000	30,000,000	300,000,000
	10,000	100,000	100,000,000	1,000,000,000
	15,000	150,000	150,000,000	1,500,000,000

Thus, starting with 1,000 protospores, the number of parasites required to produce fever, namely 150,000,000, will not be attained until the twelfth day. Starting with 2,000 protospores, this number will, it is true, be attained on the

tenth day, two days earlier; but after this point we shall have to increase the number of protospores originally injected up to 15,000 before we can reduce the incubation period by another two days. In other words, if we estimate correctly, 2,000 protospores should give as long an incubation period as 14,000 protospores, or seven times as many, would give. That is, it makes little difference to the patient whether he is bitten by one or by seven mosquitos, each of which injects 2,000 protospores. I suppose that even two or three protospores, if they survive, would set up infection.

I have assumed that *P. vivax* increases tenfold at every generation, but this is a mere guess. The following table gives the first seven powers of some natural numbers and also the proliferation of a single plasmodium according to various rates, from fivefold to twentyfold. The party-line shows where the numbers, if multiplied by 1,000, would reach the fever-point.

Powers of Natural Numbers.

5	25	125	625	3,125	15,625	78,125
6	36	216	1,296	7,776	46,656	279,936
7	49	343	2,401	16,807	117,649	823,543
8	64	512	4,096	32,768	262,144	2,097,152
9	81	729	6,561	59,049	531,441	4,782,969
10	100	1,000	10,000	100,000	1,000,000	10,000,000
11	121	1,331	14,641	161,051	1,771,561	19,487,171
12	144	1,728	20,736	248,832	2,985,984	35,831,808
13	169	2,197	28,561	371,293	4,826,809	62,748,517
14	196	2,744	38,416	537,824	7,529,536	105,413,504
15	225	3,375	50,625	659,375	11,390,625	170,859,375
20	400	8,000	160,000	3,200,000	64,000,000	1,280,000,000

This table may prove useful for estimating the average rate of increase of the various parasites, exact experiments upon which are much needed. I think it possible that more spores may be produced early in the infection than later; and also that there is likely to be a much smaller mortality among the spores at first than there is later, when the germicidal powers of the host become (hypothetically) stronger

The following table gives the incubation periods actually found in the fundamental inoculation experiments described in sections 14 and 16, omitting the doubtful results. The cases are numbered as in those sections.

1. *P. MALARIAE: Blood Inoculations.*

Case	10	13	14	16	23
Quantity of blood c.cm.	3	?	1	1	4
How injected	ven.	ven.	cut.	cut.	ven.
Incubation (days)	12	15	17	12	11
Case	29	30	35	36	37
Quantity of blood	2	2	4	4	4
How injected	cut.	cut.	cut.	cut.	cut.
Incubation	16	11	25	25	25

(*No mosquito inoculations.*)

2. *P. VIVAX: Blood Inoculations.*

Case	8	9	18	19	20	21
Quantity of blood c.cm.	?	1·5	2	2	2	2
How injected	ven.	ven.	?	?	?	?
Incubation	11	11	12	12	10	10
Case	22	34	41	42		
Quantity of blood	3	0·2	3	2		
How injected	ven.	cut.	ven.	ven.		
Incubation	6	21	3	5		

3. *P. VIVAX: Mosquito Inoculations.*

Case	9	10	11	24	25	27
No. of mosquitos	?	1	?	?	?	6
Incubation	21	15	25	17	17	15

4. *P. FALCIPARUM: Blood Inoculations.*

Case	15	17	24	25	26	27
Quantity of blood	1·5	1·5	0·25	2	5	0·75
How injected	cut.	cut.	cut.	?	?	?
Incubation	15	18	12	3	4	5

4. *P. falciparum: Blood Inoculations.*

Case		28	31	32	33	38	39
Quantity of blood		0·2	2	?	?	1·5	1·5
How injected		?	cut.	cut.	cut.	cut.	cut.
Incubation		4	15	6	10	30	6
Case	40	43	44	45	46	47	48
Quantity of blood	1·5	1·5-4·0	1·5-4·0	1·5-4·0	1·5-4·0	1·5-4·0	1·5
How injected	cut.	ven.	?	?	?	?	ven.
Incubation	17	2·5	4	7	4	3	5

5. *P. falciparum: Mosquito Inoculations.*

Case	26	30	31	33	34	35
No. of mosquitos	?	6	7	2	1	2
Incubation	15	6	10	12	14	11

From these figures we collect the following extremes and averages for the incubation period in days *per case.*

	Lowest	Average	Highest
Quartan, blood inoculation	11	17	25
Tertian, blood inoculation	3	10	21
„ mosquito inoculation	15	18	25
Malignant, blood inoculation	2·5	8	30
„ mosquito inoculation	6	11	15

As we might expect, *P. malariae* gives the highest average incubation period, *P. falciparum* gives the lowest, and the mosquito inoculations have distinctly longer periods than the blood inoculations.

The short periods given by some of the blood inoculations may be easily explained. When discoverable in ordinary thin-film preparations, the parasites may number anything from 50 per c.mm., or less, to 200,000 per c.mm., or more. In the latter case there will be 200,000,000 parasites per c.cm., so that if 1 c.cm. or more of such blood is injected, the subject should receive enough parasites to produce fever at once (by

hypothesis) in a non-immune person; and this has almost occurred in some of the cases. But, besides the number of parasites contained in the inoculated blood, there is another question requiring consideration; that is, whether many of them are not killed in the syringe. In spite of the experiments of Bein and Sacharoff (18-21 and 24), I think that this is possible, or even likely. The experiments of Celli and Santori (35-40), in which various sera were mixed with the inoculated blood, generally show long incubation periods, probably due to such destruction of the parasites. It is unfortunate that the inoculated parasites have not been counted in a single one of these experiments.

We need not refer here to incubation periods determined by observation of natural infections.

19. The Further Progress of the Invasion.—We now endeavour to trace the progress of the invasion after the commencement of the fever.

(1). *Increase of the parasites.*—In assuming that about fifty plasmodia per c.mm. are required to produce the first illness, we must remember that this applies only to a single set of parasites. If the patient contains several sets, each sporulating on different days, the total number of plasmodia should by hypothesis be several times larger. Moreover, it does not follow, if fifty plasmodia per c.mm. are sufficient to produce the first attack, that they will suffice to cause subsequent ones after the patient has become (hypothetically) habituated to their poison. Lastly, the various species may vary in "virulence."

As I have said, this number, which may be called the *pyrogenous limit*, is merely a rough estimate of mine. Long researches are required to obtain a more exact figure from observation, but it may be useful to give a few of the first counts made by D. Thomson and myself in cases in Liverpool, that is, *not* in fresh infections:—

Case.	Date.	Temperature.	Parasites per c.mm.
1. Quartan	24.1.1910	39·7 C.=27 P.[1]	1,500, mature
"	25.1.1910	slight rigor (? temp.)	83, mature
"	26.1.1910	37·4 C. slight rigor=4 P.	150, mature
"	27.1.1910	normal=0 P.	36, mature
2. Malignant	10.1.1910	39·6 C.=26 P.	58,000, young forms
3. Malignant	11.1.1910	41·0 C.=40 P.	300,000, young forms
4. Malignant	11.1.1910	38·0 C.=10 P.	15,000, young forms
5. Malignant	13.1.1910	38·7 C.=17 P.	55,000, young forms

In the malignant cases, the young forms, being merely the offspring of the mature parasites which caused the corresponding forms and which were at the time in the inner organs, do not give the number of the parent forms ; but we may form a rough estimate of the latter by dividing the number of young forms by ten. The quartan case (triple) gives some justification for the figure which I have tentatively selected as the pyrogenous limit (addendum 1).

From this point, to judge from the experimental inoculations and also from general clinical experience, the progress of an untreated case is generally that the parasites continue to increase in numbers till they may reach the figure of several hundred thousands per c.mm.

No accurate computations of the increase of the parasites seem to have been attempted even where their numbers are large enough for easy enumeration. If they increase by 10 at each generation, they should multiply from 50 to 5,000 in four days for tertian parasites, but there is reason to suppose that the increase now begins to be considerably checked.

(2). *The maximum number of parasites.*—I do not consider that 200,000 young malignant parasites per c.mm. is exceptionally high. In Mauritius we found in a fatal case 12/100 of the haematids infected. Several authors record 30/100, and Rogers

[1] Here P. stands for the *Pyretic Scale* which I propose for pathological work. It is merely the *Centigrade Scale* between 37° and 47° divided into 100 parts. Thus :—

0° P. = 37° C. = 98·6° F. = 29·6° R. = normal human blood temperature

30° P. = 40° C. = 104° F. = 32° R. = high fever.

[1908, p. 222] mentions a fatal case "with more parasites than corpuscles." Three or four parasites are frequently found in one haematid. Similar copious infections are the rule in genus *Haemoproteus* (*Halteridium*) of birds.

There are innumerable references to this part of the subject in the literature, but none of them are exact. Many authors give the proportion of their cases in which they have detected the plasmodia in a single thin-film specimen—that is, I suppose, in numbers over about 50 per c.mm. Rogers states that out of his successful cases they were detected in a minute or two in 78% and in over five minutes in 10% [1908, p. 220]; but he failed to find any parasites in 10-20% of undoubted malaria cases. Other observers have had still more failures. In Liverpool we nearly always succeed if fever is present, but the blood is generally distributed to a class. It is obviously a question not only of the number of parasites, but of skill; and I should add that not all of those who write on the subject are as expert as may be imagined.

According to the classical theorem of Marchiafava, Celli and Bignami, the sporids of *P. falciparum* tend to retire to the inner organs after reaching a certain size. I remember a case where I undertook to demonstrate the plasmodia to a sceptic, and found for the purpose a case swarming with young malignant parasites. A few hours later, however, when the sceptic saw the case, they had all vanished! Nevertheless, I think that this theorem requires to be better verified by strict numerical work, as it is possible that much of this supposed disappearance from the peripheral circulation is due to the death of the organisms (addendum 1).

(3). *Limitation of the invasion.*—Clearly, if the parasites can continue to multiply for ever at the original rate, every untreated case would infallibly die. We are therefore obliged to admit that something happens to check the invasion. The subject belongs to the pathology of malaria, but we should note the following points.

The arrest of the invasion may be due to one or both of two causes. Either the parasites themselves begin to lose their power of reproduction, or else they or the body produce something which opposes them. I doubt the former hypothesis, because the same number of spores appear still to be formed; because the parasites do continue to reproduce in smaller numbers for months, or even years; and because in some cases they actually do continue multiplying until they kill the patient. Whether they are destroyed by their own toxins, or by some germicidal substance produced by the host, is a question which deserves much more attention than it has received. The idea that they are destroyed solely by the phagocytes is no longer generally accepted.

Whatever it is, the germicidal substance appears usually to increase in power with the number of the parasites, and therefore to check the invasion at its height. On the other hand, the failure of many inoculation experiments suggests that some persons possess such a substance from the first—unless the failures have been due to some unseen error. Out of six birds which escaped infection in my mosquito inoculations, one (the only survivor) was infected on a second trial; and the same thing happened in some of the human experiments.

The literature contains many references to the sudden disappearance of a whole generation of plasmodia — not in consequence of quinine, but frequently following rest in bed, good food or shelter from heat. In the quartan case mentioned in subsection (1), a large brood followed the sporulation of 24th January, but of these, without quinine, only about 36 per c.mm. reached maturity three days later. Many observers describe the appearance of death—loss or change of staining capacity, and so on—among the parasites on such occasions.

(4). *The illness is due to a toxin.*—This point also belongs to the pathological side of the subject. Omitting reference to the older hypotheses, it is now almost certain that the patient's fever is connected with the discharge of some toxic substance

from each mature sporid at the moment when its spores are scattered in the serum. Some of the older inoculation experiments made with blood taken from a source in the state of rigor—that is, at the moment when the sporids are breaking up—were followed by an immediate slight reaction, suggesting that that blood contained a toxin as well as parasites. These results are, however, scarcely definite enough to prove the point, especially as the inoculation of healthy blood is sometimes followed by such a reaction. But the following excellent experiments of Rosenau, Parker, Francis and Beyer [1904] were much more decisive.

Case 1. At Vera Cruz, Mexico, at noon on 27th October 1903, 100 c.cm. of blood were drawn from F. Martinez, suffering from a declining paroxysm of *P. falciparum*; temperature 38·2° C. (= 12 P.); parasites, young sporids and also gametids. The serum of this blood was separated, diluted with an equal part of salt solution, passed through a Chamberland B. filter (tested), and injected (20 c.cm.) into J. Ojeira, and (equivalent) into L. Peredo. Neither subject showed any symptom.

Case 2. At the same place, at 12.30 on 6th November 1903, A. Mendez was suffering from a severe benign-tertian rigor (double infection); temperature 39·1° C. (21 P.), rising to 40·2° C. (32 P.). At that moment 125 c.cm. of his blood were drawn and defibrinated. To 25 c.cm. the same quantity of salt solution was added, and the mixture was passed through the same filter. The filtrate had no figured elements, but showed a red tinge, and 9 c.cm. of it were injected at 1.40 P.M. into the right basilic vein of L. Peredo. Thirty-five minutes later the subject "began having chilly sensations and headaches, and presently went to bed covering himself with his blanket (2.25 P.M.). Five minutes later (2.30) he was having a violent chill, his teeth chattering so that we could not trust the thermometer in his mouth." Patient was pale and vomited. Chill lasted to 3.15 P.M.; vomited again at 3.30; fever rose

rapidly to 38·7° C. (17 P.) at 4 P.M., nearly normal at 10.30 P.M. Authors put the duration of the paroxysm at about eight hours.

Case 3. At same place and date, at 2 P.M., 2 c.cm. of the same blood of A. Mendez, mixed with an equal volume of salt solution, but unfiltered, were injected intravenously into J. Ojeira. Subject "reacted within an hour, with a slight rise of temperature and nausea, and four days afterwards developed a typical malarial paroxysm, with many tertian parasites in his peripheral blood." Typical fever (double infection) with a few parasites at 7 A.M. on 10th November. (Case 51 of section 14.) In this case the preliminary rise of temperature, which began within an hour after injection, reached 37·9° C. (9 P.) and lasted five hours.

Thus, both the subjects inoculated with the blood of Mendez taken during rigor had attacks of fever similar to that of the source; but the first subject inoculated with filtered blood did not become infected, while the second subject inoculated with unfiltered blood became infected with the same parasites as in the source.

Unfortunately, no estimate is given of the number of parasites in the inoculated blood. The infection of Mendez was, however, said to be "heavy"—let us suppose 10,000 parasites per c.mm. We may thus calculate:—

The toxin of 45,000,000 benign tertian parasites in 4·5 c.cm. of blood caused eight hours' fever, reaching to 38·7° C. (17 P.) in Peredo.

The toxin of 20,000,000 of the same parasites in 2 c.cm. of blood caused five hours' fever, reaching to 37·9° C. (9 P.) in Ojeira.

The corresponding ratios, namely, 4·5/2·0, 8/5, 17/9 (taking 0 P. as the normal temperature), are not dissimilar. The pyrogenous limit here suggested is lower than my arbitrary figure of 150,000,000; but I may have underestimated the infection of Mendez, and the weights of the subjects are not recorded.

The corresponding paroxyzm of Mendez himself rose to 40·1° C. and lasted five hours; but his was an old case, and cannot be compared with the others.

We do not know the nature of the toxin, but I have always thought it likely to be some soluble constituent of the plasmodin.

It is generally supposed that the various species of plasmodia produce toxins of different strength (or virulence); but we should await better proof of this.

(5). *Antitoxins.*—Just as the body certainly produces some germicidal property capable of limiting the number of the parasites, so it must certainly produce some antitoxic property capable of destroying, or at least eliminating, their poison. We do not know its nature, but infer for the following reasons that its power increases with time:—

(1) From the time of the ancients it has been observed that malarial fever begins with *remittent* fever, due to the overlapping of the paroxysms, and ends with *intermittent* fever, due to the shortening and separation of the paroxysms. This shortening of each attack of fever is not always accompanied by a decrease in the number of parasites, and must therefore be due to something which reduces their effect on the body.

(2) Old cases, especially children, often scarcely feel the paroxysm, although considerable fever may be present.

I have observed in many untreated cases that the number of parasites continues to increase, although each attack of fever tends to become more and more mild. This suggests that the antitoxic power of the blood tends to be developed sooner than the germicidal power.

20. The Decline of the Invasion.—(1). *The period of regular paroxysms.*—It is not easy to find untreated cases for study, but from such as I have been able to examine or to read about,

I infer (subject to correction by more exact numerical methods) that the following course of events occurs :—

After reaching a figure of say 50 or 500 per c.mm. the parasites tend to increase more slowly owing to the increasing germicidal power of the blood. That is, while each sporid still produces (?) the same number of spores, a greater and greater number of the young parasites are killed ; so that the invasion tends to reach a limit of something like 1,000 to 10,000 mature sporids at every generation. But this limit may not be reached perhaps for some weeks.

In the meantime the increasing antitoxic power of the blood tends to shorten each paroxysm more and more ; the fever, at first remittent, breaks up into a series of intermittent attacks. Thus, though at each paroxysm the total number of parasites may be slightly increased as that number approaches the highest limit, yet the parallel increase of the antitoxic power annuls the effect of the parasitic increase, so that the patient now often suffers from a long series of paroxysms of almost equal intensity, which give the typical classical picture of a malarial fever.

This period may last, I think, for some weeks, but of course the process may be subject to modification. Thus, food and rest may help the patient, while in some cases death may occur in spite of quinine from an unlimited propagation of the parasites. Fortunately, however, the period ends abruptly in most cases even without quinine. The paroxysms become very mild, and then, suddenly, the sporids undergo an immense fall in numbers ; and this stage of the disease ends.

The collateral incidents and symptoms are minutely described in pathological books, and it is necessary here to refer only to some important points.

According to the elementary law of Golgi, a patient may contain, not only one set of parasites sporulating every second, or every third day (as the case may be), *but two or three sets* sporulating on different days. He may also contain sets of

parasites of different species. The rule generally accepted is that *each set of parasites continues its own evolution independently of other sets which may be present.* But much more precise work requires to be done on this point, and on the following:—

(*a*) The exact increase of severity, if any, of a paroxysm due to the simultaneous sporulation of two sets of parasites of different species on the same day.

(*b*) Does one set of parasites affect the body-reactions against another set of the same or of a different species?

(*c*) Do different sets of the same species tend to coalesce?

(*d*) Explanation of the (apparent) extinction of one set long before that of another.

The presence of different sets may obviously be ascribed to inoculation of the patient on different dates. This matter has already been touched upon, but not exhaustively, in some of the inoculation experiments, especially in those of Di Mattei, Elting and Jancsó.

Besides fever, the parasites produce *anaemia.* This has often been measured absolutely, but never exactly in correlation with the number of parasites. It is supposed that the toxin of the plasmodia destroys many of the haematids which are not mechanically broken up by the parasites themselves. More exact researches are also required regarding the absolute and relative *leucocytic variations* in correlation with the number of plasmodia.

The *different species* produce paroxysms of somewhat different type and intensity. Many authors quote figures, but without correlation with the number of parasites. Thayer and Hewetson [1895] give 11·8 hours as the average duration of the single benign tertian paroxysm, about ten hours as that of quartan paroxysms, and twenty to twenty-one hours as that of the malignant paroxysms. The *massing* of the malignant paras tes in the inner organs, and many other details, are

pathological matters. The *enlargement of the spleen and liver* will be dealt with in section 22.

(2). *The appearance of sexual forms.*—With the quartan and benign tertian parasites, these forms begin to be seen very early after the plasmodia become numerous enough to be found at all. It is very doubtful whether they are or are not produced in broods at any given moment during the course of the invasion or during the development of a single generation; and still more doubtful how long a single gametid can live in the blood of the patient. The opinions of many writers may be quoted, but, unfortunately, they are only opinions. Schaudinn's case will be referred to presently. The malignant gametids are much more easy to study. They begin to appear about a week after the onset of the fever and may remain circulating in the blood for several weeks. We do not know what determines their appearance, but the asexual forms and the fever frequently vanish when they appear (which may often be due to the fact that quinine influences the sporids more than it affects the crescents). In untreated cases, however, we may often find both forms together for many days, though the sporids are apt to be scanty. In such cases there may be slight oscillations of temperature, which some writers have attributed to the crescents. Often, also, the sporids may be still present in large numbers though they are too few to be detected. In infected sailors in Liverpool we often find crescents with great certainty, unless the earlier infection is cut short with quinine. But many writers complain that in certain localities they cannot find them as frequently as might be expected, and I have had the same experience in India and West Africa. To explain such cases I have surmised that the production of sexual forms may be largely influenced by season —that they may be produced abundantly for a few weeks, but only at that time of year when the local carrying Anophelines are most abundant. Caccini [1902] says that several observers have noted the absence of crescents when the new

malaria season is imminent, and that he himself has never found crescents in Italy from April to June. For another example, I found very few crescents at the foot of the Darjeeling mountains in 1898 at a place where they had been frequently seen by another observer a few months earlier. But this is a mere conjecture, and one which requires long study for verification; and it scarcely explains why sailors in Liverpool have crescents so regularly. The early use of quinine—to which sailors are not addicted—may also explain the paucity of crescents in many cases. Lastly, it is possible that the sexual forms are produced only during the earlier stages of an infection—that in an old infection, when, so to speak, parasites become worn out, they no longer produce sexual forms, just as the oldest broods of human cells fail to produce them. In my experience crescents are common during the first few months of an infection, but comparatively rare at later stages. Failure to find "flagellated forms" (spermatophoria) must be generally due to faulty technics.

(3). *The period of rallies and relapses.*—We have seen then that the period of regularly repeated paroxysms generally ends after a few weeks, even without treatment, in a great decline in the number of sporids. The gametids, especially the crescents, may still remain numerous, but the asexual forms which cause the illness diminish so much that they fall below the pyrogenous limit and cannot easily be found.

The patient now improves in health. His paroxysms cease, his haematids increase, his spleen diminishes, and he begins to gain flesh again. I call this the rally.

From this point two things may happen. The patient may recover completely, or—without reinfection—he may suddenly suffer from a *relapse.*

In the relapse all the old symptoms of the period of regular paroxysms, sometimes less in intensity and sometimes worse, recur. The fever may recommence with the remittent form, and may then, as before, become intermittent. The

anaemia and the splenomegaly—not yet completely recovered from since the first attack—will again increase, and to a greater degree than at first. Large numbers of sporids will again be found in the blood, and may be followed as before by crops of gametids. Finally, in most cases, even when untreated, recovery, accompanied by a great decrease of sporids, will occur again.

This is followed by another rally, which in its turn *may* be followed by another relapse—and so on, indefinitely, for months or perhaps years.

Such, in my opinion, is the normal course of malaria, whether untreated or badly treated. But as the former kind of case can scarcely ever be observed, my opinion is based, not upon continuous observation of many cases, but upon correlation of different periods in different cases. I think, however, that most students of tropical malaria share that opinion.

During the whole of this period the *emaciation*, *anaemia*, *splenomegaly* and secondary symptoms tend to increase with each relapse and to diminish with each rally. But the increase seems at first to be generally greater than the decrease, though by a diminishing increment. Thus, after some months the patient tends to reach a condition in which these symptoms arrive at something like a fixed limit—considerable emaciation, anaemia, splenomegaly, oedema, dyspepsia, etc. This condition is well known as that of *chronic malaria*, and is only too frequently seen in malarious places, especially among children.

In most cases, especially in children, after the fixed limit has been reached a general *improvement* sets in. After all, the disease is essentially a benign one. Each relapse now affects the patient less and less, and the secondary symptoms begin to decrease more with each rally than they increase with each relapse. After an unknown average duration, complete recovery certainly occurs in the large majority of cases, without any serious treatment. Such cases are said to have become partially *immune*.

On the other hand, *death* occurs in many as the result of "pernicious paroxysms," intercurrent affections such as dysentery and pneumonia, or general weakness assisted by poverty caused by inability to work.

Of course this picture—or rather sketch—is modified by many conditions. Patients who have already become partially immune during childhood or after many attacks, suffer much less on reinfection; and good food and change of climate benefit the case, while complications such as dysentery, sprue and ankylostomiasis, have the opposite effect. The most usual modification is that caused by inadequate treatment, and I continue to see many such cases in Liverpool. The history is always the same. The patient has been instructed to take quinine during his attack, and for a week or two afterwards. This is insufficient, and the inevitable relapse occurs. But the patient seldom falls into the extreme condition of wholly untreated cases. The drug has not extirpated the infection, but it has checked each relapse as it occurs, and has controlled the anaemia and splenomegaly. Such especially is the condition of the infected sailors of whom we see so many in Liverpool. They still have relapses, but the spleen is not markedly enlarged and the anaemia not very pronounced.

Works written on malaria in temperate and often more civilised countries do not always give a complete picture of the disease as it occurs among poor natives in the tropics. This is due to the fact that the more general use of quinine in the former tends to abbreviate the course of the infection. On the other hand, the great majority of cases in the countless malarious villages of the tropics are cases of chronic malaria —untreated, subject to frequent reinfections, and with anaemia, enlargement of the spleen and numerous relapses. In temperate climates, our cases are mostly in the early *period of regular paroxysms*; in the tropics they are mostly in the long-continued *period of rallies and relapses.*

(4). *The parasites during the rallies.*—The general observa-

tion that the degree of fever depends *cceteris paribus* upon the number of parasites suggests that when there is no fever the parasites are likely to be at least scanty. Hence, as is to be expected, in the rallies between relapses few sporids, and indeed very often none at all, can be discovered in the small quantities of blood examined under the microscope—though, of course, the gametids (which do not seem to produce fever) may still abound. Nevertheless, when the relapse occurs, the sporids again appear in detectable or large numbers. What happens to them when they disappear and reappear in this manner?

We can easily observe that the number of parasites, when they are numerous enough to be found, often varies largely. It is therefore equally easy to infer that when we can no longer find them, this is due simply to the fact that they have become too scarce to be found. But many writers seem to think that when they cannot find sporids none exist, and have therefore sought other explanations of the disappearance and reappearance referred to.

Years ago Marchiafava and Bignami suggested that when they disappear for long periods the parasites may become encysted somewhere in the inner organs; or that they may die out altogether, and that the relapse may be caused by spores which escape from phagocytes in which they have been living in a dormant condition. No evidence has been given for these views; yet it should be easy to obtain in the malaria of birds, which remain infected for months. In 1898 I examined many birds in vain for the supposed encysted forms.

Golgi, Mannaberg and others thought that crescents keep the infection alive during the rallies. Thus Mannaberg said [1894, p. 302]: "These relapses in localities free from malaria are to be explained only by the persistence of parasites possessing certain powers of resistance (probably the crescentic bodies) within certain tissues." But why are any special powers of resistance required; and how explain relapses with the parasites which do not produce crescents? Many authors,

working, I believe, with unstained films, have described and figured sporulation of crescents; but others attributed this merely to post-mortem vacuolisation. We have never found sporulating crescents in stained films in Liverpool, though numbers of these bodies are examined in class. B. Grassi [1901] suggested that the relapses are caused by parthenogenetic reproduction in the gametids.

This idea was followed up by F. Schaudinn [1903]. A patient who had suffered long from *P. vivax* was attacked on 29th April and 1st May, and was found to contain both sporids and gametids. A rally now occurred, during which daily examination disclosed *varying* numbers of gametids only. On 25th May these parasites were *more plentiful*. Next day curious changes were noted in the female gametids, suggesting that they were producing spores similar to those ordinarily produced by the asexual sporids. The author considered this to have occurred independently of fertilisation by the male gametids, and to be due to parthenogenesis. The same day (26th May) the patient had a slight rise of temperature to 38·4° C., and in the evening ordinary young sporids were found. Next day *only* these forms occurred. On 28th May there was a typical attack with temperature reaching 40·75° C., and with the usual sporids. Next day only gametids were again found. The author carefully described the parthenogenetic forms and traced the corresponding changes in the nucleus.

These observations were hailed as an important discovery by many writers, especially zoologists. They have been partially repeated by Maurer [1902], Bluml and Merz [1908], Harrison [1909] and others; and many of the younger and more confident workers have spent much time in attempts to verify them, or have seen objects like those described by Schaudinn. But the objects twice seen by Maurer are not in any way proved to be parthenogenetic forms; while those observed in six preparations by Bluml and Merz are supposed by the authors to be more probably cases of production of gametids by gametids—another

hypothesis; and these authors do not agree with Maurer. Harrison admits that the supposod "gametoschizonts" seen by him may be merely double infections of haematids by a gametid and a sporid. Craig [1906] thinks that "intracorpuscular conjugation" may maintain the infection.

It is easy to see under the microscope objects which suggest this or that hypothesis, but only the most patient and long-continued labours suffice to prove the truth. Schaudinn's case seems to me of doubtful value. I note especially that between 1st and 25th May the number of gametids *varied*, and was increased on the latter date. But this variation suggests that they were being produced all the time. The gametids are supposed to be produced from ordinary spores; so that we are forced to infer that a number of sporids, some of them generating gametids, were present in the patient's body, although they were too few to be detected in the small quantities of blood examined by the author. On 25th May they probably increased in number sufficiently to induce a slight attack of fever, and were then mistaken for parthenogenetic gametids. On 28th May they produced a typical attack; and that is all. The supposed nuclear changes were reported on evidence of no great value. The cells were not actually observed undergoing the development which the author describes. He merely inferred the existence of the development from a study of different cells in what he thought were different stages of that development. Worst of all, no numerical estimates are given. The same author enunciated many other hypotheses on similar evidence. I doubt whether parthenogenesis occurs with the parasites of malaria, for the following reasons:—

(*a*) If it occurs in one species it ought to occur in all. It ought therefore to be easily observed in birds' malaria, and in crescents. I have been examining the latter for fifteen years and have never seen in them anything suggesting parthenogenesis. Yet they possess a definite and uniform outline which should

be much modified by such cellular changes as Schaudinn described. A. Carducci [1905], though favourably inclined towards the parthenogenetic speculation, failed after careful search in finding any evidence of it in crescents.[1]

(*b*) I have frequently seen relapses occur in cases of *P. falciparum* in which no crescents at all could be found. Carducci notes the same (see section 65).

(*c*) In four experiments (see end of section 14) blood containing numerous crescents only was injected into healthy persons by capable observers without producing any infection whatever. Yet I showed in 1895-1896 that crescents live on ice and for hours under vaseline. Why then, if Schaudinn's hypothesis is true, did they not infect any of these four persons?

(*d*) If relapses are caused merely by some natural development in the parasites we should expect them to occur more or less independently of the state of health of the host—which does not seem to be the case (see subsection 6).

(*e*) If it occurs at all, parthenogenesis ought to be common enough to be demonstrable with ease and certainty.

This subject is of great importance in connection with the prevention of malaria. Crescents withstand much medication with quinine, and if they keep the infection alive in patients, it follows that quinine loses much of its value as a public prophylactic against malaria. But, while I am quite ready to accept any evidence for it which may be offered, at present I do not think that the Grassi - Schaudinn speculation is even nearly proved.

The fact that the parasites disappear at each rally and reappear at each relapse is explained quite easily and sufficiently. In the rallies they become too few to cause fever or to be

[1] H. M. Neeb [1909] in a paper just to hand shows careful, but I think still quite inconclusive, work in favour of parthenogenesis in crescents.

detectable in the small quantities of blood examined; in the relapses they become numerous again. I cannot see that a simpler or more satisfactory explanation is required. We observe the same process in relapsing fever, tuberculosis, trypanosomiasis and other diseases.

A good writer once said: "It is hardly conceivable that it (the parasite) should remain in the general circulation, passing through its ordinary cycle of existence, without causing any symptoms whatever. Further, the failure of repeated examinations of the blood of patients who have previously suffered from malaria to reveal the presence of the parasite renders this most unlikely." It is impossible to accept such a train of thought. If 150,000,000 sporids cause but little fever in a man of medium weight, 1,000 or 100 should cause none at all, and yet will be quite sufficient to keep the infection alive in him. But if the parasites are so few, what chance has the medical man of finding a single one? If even 1,000 parasites are present in a man of 10 stone weight, the chances are that we must search 15,000,000,000 haematids before we find one parasite. At the rate of 10,000 haematids a minute we shall have to search for 1,500,000 minutes, or for twelve hours a day for more than five years, before we succeed. Or we may put it in this way—that 150,000 examinations, each of ten minutes' duration, must be made.

In seven autopsies on cases of "latent malaria," C. F. Craig [1909] found only the ordinary parasites in the spleen. Four of these contained malignant parasites, yet no crescents at all were found in the spleen.

(5). *Probable cause of the rallies and relapses.*—According to this simple hypothesis (which I will continue to adopt until a better one is established) the rallies are probably due to a great destruction of the parasites by some germicidal power of the blood, or of their toxin by some antitoxic power, or more generally to both. And the relapses are probably due to decrease in both of these factors of resistance.

It is not within our province to discuss these factors now. The general experience that many cases improve without treatment, and merely after good food and rest in bed, proves that such factors exist; and, as already stated, if they did not exist every case would certainly perish.

Many diseases — tuberculosis, trypanosomiasis, relapsing fever, leprosy, etc.—exhibit such variations in intensity. We picture to ourselves a long struggle between the invaders and the opposing force — first one side triumphs and then the other, and death or recovery finally ends the contest.

Just as rest and good food encourage the resistance, so, probably, anything which weakens the patient—fatigue, chill, heat, dissipation, other sickness — tends to encourage the parasites. Educated patients often declare that their fever is brought on by such causes. Military surgeons recognise that when infected troops are despatched upon arduous military duties numbers of them begin to fall sick at once, even though it may not be the season of fresh infections. Travellers and planters complain of the same thing as regards their porters or coolies; and medical men notice the frequency of malarial relapses after typhoid fever, venereal diseases, child-birth, accidents, etc.

I think that external heat probably tends to encourage relapses. Although the temperature of the body remains much the same, the parasites may possibly be stimulated by the heat in some way, owing to the fact that warm climates are specially suitable to them and to their dissemination by mosquitos. Thus in my experiments on birds in 1898, I noticed that when the birds were taken to the cool climate of the Himalayas their parasites greatly diminished, and when they were brought back again to Calcutta their parasites increased again. In the hot weather in Calcutta 11% of the sparrows examined contained *P. danilevskyi*; but early in the cool weather few were found to be infected. This point requires exact experimental study. A. Caccini [1902] thinks

that *excessive heat* does not have much effect, but notes the frequency of relapses in benign tertian in spring; and it seems to me that the onset of warm weather rather than the heat itself is most likely to be the encouraging factor, since the germicidal power is probably heightened after the first relapses in spring. It is interesting to note that in India there is generally a small but well-marked rise in the admission rates of troops at the commencement of the warm weather in March to April. For example, I take at random the Native Troops on the North-West Frontier, Indus Valley, and North-West Rajputana ("Reports of the Sanitary Commissioner with the Government of India"), and give the averages for the three years 1900-1902.

AVERAGE ADMISSIONS.

Strength	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
16,428	379	205	191	285	401	326	420	396	565	1,397	1,152	735

Here there is a marked increase between March and April, that is, at the commencement of the hot dry weather, long before the advent of the breeding season of Anophelines. It may, however, be due to the awakening of infected hibernating mosquitos (section 21).

Why, if the germicidal power is strong enough to destroy large numbers of the parasites, does it not destroy all of them? I presume that, as in other forms of life, the hardiest individuals often succeed in withstanding the opposing forces unless they are too strong; that a few of the plasmodia survive until the germicidal power is fully developed—when the entire brood is exterminated.

What is it that determines the length of the rally, or apyrexial period? Probably (*a*) the number of plasmodia left alive at the end of the previous attack, and (*b*) the strength of the germicidal power of the patient. If the parasites are reduced to about the number of protospores originally inoculated, and the germicidal power weakens rapidly, the former will now multiply again at a rate somewhat similar

to their first rate of proliferation—so that the length of the apyrexial period should be roughly equivalent to the original incubation period. If the germicidal power is strong, or is fortified by quinine, the plasmodia left alive after the previous attack should multiply so slowly that the apyrexial period may be indefinitely prolonged. Under such circumstances a few parasites may perhaps continue to breed for months in a patient, without causing the smallest symptoms, and yet, if the germicidal power is reduced for a moment, may be able at any time to multiply again sufficiently to produce another attack. The same thing probably happens with irregular or insufficient quinine medication. The long intervals which frequently occur can be explained equally well on this hypothesis.

We do not know why the resistance of the host should be affected by his general state of health or by external conditions. That such is the case affords another argument against the idea that the relapses are due merely to some normal development of the parasites. The study of immunity in malaria should yield important facts for the study of immunity in general. I merely touch upon the subject here.

Cases are often reported in which the patient during the relapses suffers from malaise, dyspepsia, and so on, rather than from fever, although the parasites are numerous enough to be found. In such, I presume, the antitoxic power of the blood has become better developed than the germicidal power. Cases in which a relapse *immediately* follows a chill or accident are explained perhaps by the hypothesis that the chill or accident has immediately reduced the antitoxic (not necessarily the germicidal) power of the blood.

(6). *Frequency of the relapses.*—All clinicians recognise that in untreated or badly-treated cases a considerable number of relapses may occur before complete recovery is established. Cases giving a history of say three to ten relapses are very common. But we are indebted to A. Caccini [1902] for giving us a very able and full analysis of this question. In

1899-1901 he was entrusted by Professor Ballori, Director-General of the Hospitals in Rome, with the care of a special department for the study of malaria, and set himself to the task of examining these questions methodically. The cases studied by him were as follows:—

Type	First Attacks	Relapses	TOTAL
Quartan	525	330	855
Benign Tertian	1,086	934	2,020
Malignant	2,275	1,429	3,704
TOTALS	3,886	2,693	6,579

Many of the cases were given quinine, so that this proportion will not accurately represent the proportion of relapses in untreated cases—which should be much larger. The author excluded all cases in which the parasites were not found. He endeavoured to ascertain as exactly as possible the history of each case, not only by enquiry of each patient, but by reference to hospitals and localities where they had been treated; he kept many cases in hospital for long periods, and he gives a careful record of his results. I will mention here only his untreated cases.

Two cases of benign tertian, untreated, were kept under observation in bed in hospital for nine months. The following table gives the duration in days of their successive relapses and rallies:—

	Rel.	Ral.	Rel.	Ral.	Rel.	Ral.	Rel.	Ral.	Rel.	Ral.	Rel.
Case 1	17	13	16	16	14	19	15	17	12	10	19
Case 2		15	17	16	15	18	20	17	6	10	9

Both patients were robust, had the same diet, and were not allowed to suffer from cold or damp or intercurrent affections. Nineteen similar cases studied for shorter periods showed the same phenomena. The author notes that out of 120 untreated cases of benign tertian, the relapses occurred more frequently in the spring (March to May). He thinks that the rallies continue from 5 to 18 days, which he calls apyrexial periods or *short interval.* But he also notes and distinguishes rallies of

long interval, lasting 3 to 4 months, which he says occurred in 152 persons who had not revisited malarious places in the meanwhile. The rally lasted under 2 months in 60 cases; under 70 days in 29 cases; 90 days in 20 cases; and from 90 to 120 days in the remaining cases.

He also quotes the case of a band of 75 Calabrian workmen who had become infected at Foggio in July 1900, and had subsequently taken quinine until December. On 8th September 1901, 14 months after the first infection, 62 of these men were exposed to a severe wetting with rain, while the remaining 13 found shelter. On 10th September and the six following days, every one of these 62 men was attacked with relapses. The intervening rallies since their last attacks had lasted for 7 months in 2 of the men; for 7·5 months in 3; for 8 months in 12; for 9 months in 10; for 10 months in 15; and for 13 months in 20. Those who had not been wetted by rain escaped. It is supposed that none of the cases had been subject to reinfection.

In quartan the author found that out of 118 cases kept without treatment relapses occurred only in 8 which had been exposed to chills and other predisposing causes. He thinks that quartan relapses chiefly as a consequence of such causes —unlike tertian, which relapses under any conditions. The length of the rally depends upon the care which has been taken of the patient; but the author quotes six cases of relapse occurring after long intervals (6 to 10 months). He says also that in some cases of experimental inoculation of quartan by him (no reference) the incubation period could be prolonged at pleasure by keeping the subject at rest in bed, etc.

Regarding malignant fever, Caccini gives the following lengths of the rallies in 44 untreated persons:—

Days . .	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
Cases . .	9	1	5	1	7	1	1	1	1	1	1	1	2	6	1	3	2

The author suggests that there is some radical difference between relapses at short and at long intervals, but I cannot

find that he proves his case. He agrees with all observers in attaching great importance to the *determining factors* of relapse, which he says are,

(1) absence of good food and health,
(2) gastro-intestinal troubles,
(3) fatigue,
(4) traumatism,
(5) sudden chills, damp, and wetting,
(6) change of climate,
(7) certain foods and medicines (?) due to intestinal disturbance,
(8) intercurrent sickness.

To these I would add :—

(9) anxiety, sorrow, shock or fright,
(10) alcoholic and other excess,
(11) sudden exposure to a tropical sun,
(12) premature cessation of quinine.

It is, of course, understood that these determinants act by reducing temporarily the host's resistance, but we do not know exactly how they do so.

Caccini endorses the common opinion that quartan and tertian, especially the former, are most prone to relapses. In malignant, the original illness is more severe and (probably for this reason) relapses are not so common. I will refer to his important results with quinine in section 23.

Celli [1901] gives a useful table showing primary cases, relapses and pernicious cases in the Roman Hospitals for 1892-1898. The following are the totals for the seven years :—

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total
Primary	864	676	724	793	809	696	2,912	5,157	4 440	3,875	3,074	1,967	25,987
Relapses	581	358	319	341	353	205	522	949	1,031	1,156	989	716	7,520
Pernicious	2	1	—	—	—	1	45	62	67	64	56	32	330

Thus, excluding the pernicious attacks, out of a total of 33,507 cases, 75% were reported to be primary and 25% relapses—that is, three to one.

Relapses are referred to in many other works, but the information given is scanty. The illness being generally slighter, patients do not often come to hospital in consequence of it; and in most cases quinine distorts the clinical picture (section 22).

Relapses after long intervals are frequently mentioned. The case of P. T. Manson (section 16, case 5), who had a relapse nine months after the original infection, followed by three months' quinine treatment, is well known. In one case at the Royal Southern Hospital I found the parasites in a patient who said that he had left the tropics four years previously (records unfortunately lost). In Britain we have many returned officials who say that they continue to have occasional attacks after return from malarious colonies—more than twenty years have been mentioned to me by three persons; but as each attack lasts only for a day or two it is generally impossible to verify the point microscopically. My father left India in 1880, but in 1889 still complained of occasional attacks. A little later I witnessed one of these—a sudden severe rigor followed by high fever and a typical sweating stage. The blood was not examined, but the paroxysm was almost certainly a quartan or tertian one. So far as I remember there was no determining factor in this case.

21. Average Duration of Untreated Infections.—Persons may therefore remain infected for several, and possibly for many years; just as some may recover spontaneously after the first series of attacks. But from the public health point of view an important question remains to be answered, namely, how long does the infection continue in untreated cases *on the average*? That is, if 1,000 persons, simultaneously infected, were to be removed simultaneously to a perfectly healthy area, where they are kept untreated, how long would their infections continue? It is impossible to say at which particular moment the last parasite dies out in a patient's blood, and no adequate researches have been made on the point. We are therefore forced to rely upon certain calculations.

The cases of malaria at Ismailia from 1900-1905 are given officially as follows:—

Years . .	1900	1901	1902	1903	1904	1905
Cases . .	2,284	1,990	1,551	214	90	37

In 1901-1902 quinine was largely distributed in the town, but after my visit at the end of 1902 the Anophelines were banished, the quinine also being continued to those who took it. Since 1905, and probably since 1902, there has been no endemic malaria, such cases as occur being found only in persons infected outside, or being merely relapses. Hence we infer, as a very rough calculation, that of the 1,551 persons who were ill in 1902, less than 1/7 (214) remained ill in 1903, and less than 1/17 (90) remained ill after two years. But here quinine expedited the recovery on one side of the account; while on the other side of it many of the cases that remained were probably due to external infection.

Another way to estimate the reduction is, I suggest, to consider the statistics of some country where fresh infections suddenly cease owing to the abrupt onset of a sharp winter and the consequent cessation of mosquito breeding and mosquito biting. I select as an example the Punjab, a large malarious area in the extreme north-west corner of India, and give the figures of admissions furnished by the annual "Reports of the Sanitary Commissioner with the Government of India."

The years 1899 to 1903 have been selected because after the latter date quinine began to be administered much more regularly. Nevertheless, subsequent figures show very similar monthly variations—as in fact are shown by the same statistics for other parts of India.

The ratios under each monthly average are obtained by dividing by it the following monthly average—so that these ratios give the monthly factor of increase or decrease in the number of admissions.

The striking regularity with which the maximum is reached in October and the minimum in February will be observed.

A. EUROPEAN TROOPS—(TABLE X.)

Year	Strength	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	TOTAL
1899	18,080	344	178	206	357	585	414	530	437	526	517	223	150	4,467
1900	16,562	132	76	110	148	245	254	349	354	600	1,364	1,087	849	5,568
1901	16,431	415	190	266	397	345	495	442	560	581	738	608	338	5,575
1902	15,636	194	118	138	212	320	276	401	600	600	646	612	251	4,368
1903	17,997	126	84	141	204	350	346	431	437	542	912	1,198	846	5,617
Average	16,941	242	129	172	264	369	357	431	478	570	835	746	487	5,119
Ratios		0·53	1·33	1·53	1·39	0·97	1·20	1·10	1·19	1·46	0·89	0·65	0·49	

B. NATIVE TROOPS—(TABLE XXXV.).

Year	Strength	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	TOTAL
1899	44,224	668	514	399	409	676	722	983	1,704	1,532	2,112	1,038	590	11,347
1900	42,311	541	282	245	285	492	611	794	1,079	1,737	4,337	2,316	1,679	14,398
1901	46,050	847	471	627	947	964	1,508	1,808	2,067	2,309	3,988	2,428	1,513	19,567
1902	43,486	797	640	482	528	856	717	1,103	1,449	1,807	1,858	1,730	994	12,941
1903	40,582	574	396	322	297	399	700	718	1,026	1,060	2,479	2,148	1,174	11,293
Average	43,330	685	461	515	593	675	852	1,081	1,465	1,689	2,955	1,932	1,190	13,909
Ratios		0·67	1·12	1·15	1·14	1·23	1·27	1·36	1·16	1·75	0·65	0·61	0·58	

C. PRISONERS—(TABLE XLIX.).

Year	Strength	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	TOTAL
1899	13,512	505	296	457	389	323	365	607	727	583	627	626	447	5,952
1900	15,550	226	210	262	445	460	412	416	643	1,276	1,864	1,542	988	8,744
1901	13,469	634	229	379	522	936	867	486	720	827	921	847	704	8,072
1902	12,782	504	371	415	470	426	321	468	636	684	818	655	655	6,423
1903	12,163	275	225	240	294	411	296	197	358	365	997	688	672	5,118
Average	13,495	429	266	351	424	511	452	435	616	767	1,045	872	693	6,861
Ratios		0·62	1·32	1·21	1·21	0·88	1·36	1·42	1·24	1·35	0·83	0·79	0·62	

For the European troops the maximum (835) is 6·47 times the maximum (129), which occurs four months later. For the native troops, the maximum (2,955) is 6·41 times the minimum (461)—almost the same. For the prisoners the maximum (1,045) is 3·93 times the minimum (266)—a smaller ratio.

Thus, for 1,000 admissions in each class during October the following numbers were admitted in February:—

	October	February
European troops . . .	1,000	154
Native troops	1,000	156
Prisoners	1,000	254

A certain amount of quinine was doubtless given to all these classes, and many of the Europeans were probably moved to hill stations after April to May. It is easy to understand the small rate of recuperation of the prisoners — a constantly changing class drawn from the lowest ranks of the population and depressed by their imprisonment. Hence I infer that the rate of recovery of the European and native troops during the four months is probably too favourable for an estimate of the same rate among a large untreated native population. On the other hand, the quinine treatment of the prisoners may be cancelled by their circumstances, so that their rate of recovery should be something similar to that of the general population. We may therefore, perhaps, assume as a rough but convenient estimate for the latter that what may be called the *reduction factor* is about 0·25 in four months—that is, that 1,000 cases should be reduced to 250 in that time by spontaneous improvement.

We observe in the statistics given above that there is an almost constant but slow increase in the admissions from February onward. Whether this is at first due to heat relapses or to infections by hibernating Anophelines (section 20) is not known; but if no such increase were to take place, we might suppose that the same rate of recovery would hold for

the rest of that year. In other words, we might suppose that any number of patients brought to England would be reduced by recovery to one quarter (0·25) every four months — that 1,000 cases would become 250, 62, 15, in four, eight, twelve months respectively, and so on. This is, I fear, a very rough estimate, but I know of no other method of making any estimate at all.

Hence we may perhaps infer that 3/4 of our untreated cases will recover in four months; 15/16 of them in eight months; 31/32 of them in a year, and so on. But these figures are based upon the gaol statistics of the Punjab—that is, upon those of natives, most of whom have probably had malaria in childhood. What may happen in the case of a completely non-immune and untreated population I cannot say. For such we might adopt a still lower rate of recovery, say 1/2 or 50% every three months; that is, 1/2 would recover in three months, 3/4 in six months, 7/8 in nine months, and 15/16 in a year. On the whole, this is probably the nearest estimate.

Celli [1901] gives the admissions into the Roman Hospitals during thirteen of the years between 1864 and 1898—numbering nearly 93,000 altogether. I subjoin the monthly totals for the reader to study in connection with the above remarks.

an.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total
673	3,732	3,906	3,921	3,575	2,553	8,844	17,678	15,203	12,755	9,531	6,621	92,992

22. Enlargement of the Spleen and Liver.—This is a condition of importance in the prevention of malaria, as it often enables us to estimate the amount of the disease in a locality. We must now note the following points regarding it.

The average weight of the spleen of a healthy man of 60 kilograms in weight is estimated to be 171 grams (Vierordt), or 2·85/1000 of body-weight. In healthy adults the edge of it cannot be felt under the ribs, but the reader should note that this can often be done in small infants.

The organ commences to enlarge during even the first

paroxysm of malarial fever, the increase in the area of splenic, dullness being quite appreciable. In the first apyrexial period the enlargement decreases, but as attack follows attack the sum of the successive increments is greater than that of the successive decrements, so that a more and more marked tumour results. Innumerable measurements have been recorded. Kelsch and Kiener [1889] state that in eighty autopsies they found the weight to vary from 400 to 1,500 grams, and to average 914 grams (chronic malaria).

I think that even small and irregularly administered doses of quinine tend to check the increase very much. Thus in untreated native children one often finds the organ an inch or more below the ribs after a few attacks, while there is frequently no enlargement in Europeans even after many attacks. In sailors who have reached Liverpool after weeks' or months' fever on the homeward voyage from West Africa, we find as a rule scarcely any enlargement. In 1895 my regiment in Secunderabad, India, was full of malaria as proved by the microscope and the clinical symptoms, but on making a spleen census by order of the authorities I found only four men with enlarged spleen out of about eight hundred. In both these instances a certain amount of quinine is or was given—not enough to cure the patient, but enough to check the splenomegaly.

The pathological changes are discussed in detail in the text-books and need not be described here. Laveran [1907, p. 391] considers the pathology and seems to think that the enlargement is due to local irritation caused by the accumulation of the parasites in the organs, which is their "siège d'élection." Others connect the enlargement with the defensive process. The arguments are far from conclusive on either side.

On the whole, from general observation, I consider that the following propositions are probably true:—

(1). That in a given number of infections of the same

duration (*a*) in children and (*b*) in adults, the children will show a greater degree of splenomegaly.

(2). That in a given number of infections of the same duration (*a*) in Indo-Europeans and (*b*) in Negroes, the former will show a greater degree of splenomegaly. I cannot speak regarding Mongolians.

(3). That in a given number of infections of the same duration (*a*) in untreated persons and (*b*) in persons of the same age and race treated even slightly with quinine, the former will show a greater degree of splenomegaly.

(4). That in a given number of infections of the same duration (*a*) in persons infected only once, and (*b*) in persons who have been subject to repeated infections, the latter will show the greater degree of splenomegaly.

(5). That the number of parasites tends to vary inversely as the degree of splenomegaly; that is, that the parasites tend to die out in persons with very large spleen.

(6). That a hot and damp climate, or insufficient food, or certain diets, or bad drinking water may possibly favour the development of splenomegaly; in other words, that the degree of splenomegaly may not always be an exact measure of the amount of malaria in a locality.

I cannot give anything approaching proof of these propositions, but merely suggest them for future consideration and investigation.

On the other hand, some general facts have been more strictly ascertained. Before considering them we should remember that many of the older observations have been invalidated by the discovery in 1903 that *kala-azar* a disease which causes great enlargement of the spleen and liver and abounds in parts of India, Algeria and other countries, is not

malaria, but is due to a different organism, *Leishmania donovani*, Laveran. Before this date the two diseases were frequently confounded, as, for instance, by myself in my report on kala-azar [1899]. If therefore we wish for accurate figures we must first ascertain whether kala-azar is present or not, or at least whether it is very prevalent. This is generally not difficult, for the following reasons:—

(1). Kala-azar is extremely fatal, whereas malaria is benign; so that the presence of the former is soon felt in a locality.

(2). The parasites of both diseases can be detected during life and *post-mortem*.

(3). The symptoms of kala-azar, though somewhat similar to those of malaria, are much more intense. The patients often have a look of suffering and melancholy not so frequently observed in malaria. I was much struck with this difference when comparing splenomegalous children in Greece and Mauritius with those suffering from kala-azar in Assam.

In the absence of this disease, then, we may generally ascribe most of the cases of enlargement of the spleen to malaria, especially if they are numerous. They are generally extremely numerous in most malarious localities where quinine is not much taken.

In 1907-1908 Major Fowler and I, assisted by the Sanitary Department and the medical men of Mauritius, made a large "spleen census" in that colony. We examined children of fifteen years of age and under, in all parts of the island. Out of an estimated total of 182,000 such children we tested 31,022, and found enlargement of the spleen in 10,595, or more than one-third (34·1/100). Moreover, in 30,137 we recorded the degree of enlargement, and found that it was small in 4,381, or 14·5/100; medium in 3,479 or 11·5/100; and great in 2,566 or 8·5/100—a total of 10,426 with enlargement.

This differentiation of degree suggests the possibility of

obtaining a figure which I call the *average spleen* index. Taking a normal spleen at 171 grams, a very large spleen of 1,500 grams would be about nine times that size. We may therefore define a *small* enlargement as being about three times the normal size; a *medium* enlargement about six times the normal; and a great enlargement about nine times the normal—the same ratios holding for children also. Thus in order to find the average spleen, we multiply the numbers under each heading by the size-ratio, add the results to the number of children with no enlargement, and divide by the total number of children examined. We thus have $(19{,}711 + 4{,}381 \times 3 + 3{,}479 \times 6 + 2{,}566 \times 9) \div 30{,}137 = 2·54$ the *average spleen* for Mauritius (section 31).

From these and other data I compiled the following tables [1908] showing the relation between the spleen rates and altitude, and the spleen rates and the general *death rates* in Mauritius.

SPLEEN RATES ACCORDING TO ALTITUDE (MAURITIUS).

Altitude in Feet.	Children examined.	Spleens.				Total with spleen.	Spleen rate.	Average spleen.
		1	3	6	9			
100	5,210	2,623	1,030	732	734	2,587	49·6	3·20
200	4,843	2,593	813	765	633	1,250	46·4	3·16
300	3,559	2,208	616	457	258	1,351	37·6	2·59
400	2,817	1,432	651	473	262	1,386	49·2	3·01
500	1,246	786	193	177	90	460	36·9	3·48
600	830	518	106	128	78	312	37·5	2·74
700	1,963	1,373	227	235	122	590	30·0	2·27
800	1,398	1,241	80	41	36	157	11·1	1·65
900	624	478	52	48	46	146	23·4	2·14
1,000	972	901	47	11	13	71	7·3	1·26
1,100	782	736	18	9	7	46	5·8	1·14
1,200	858	751	49	28	19	107	12·4	1·28
1,300	130	124	...	...	...	6	4·6	...
1,400	1,991	1,556	185	160	90	435	21·8	1·94
1,500	612	586	10	12	4	26	4·2	1·19
1,600	112	105	3	4	...	7	6·2	1·31
1,700	...	...	...	...	...	...	...	...
1,800	765	740	13	6	5	25	3·2	1·13

SPLEEN RATES COMPARED WITH DEATH RATES BY DISTRICTS—(MAURITIUS)

Class.	Districts.	Death rates Average 1905-1906.	Spleen rates.	Average spleen.
Estates only	Pamplemousses	37·3	39·6	2·62
	Riviere du Rampart	30·7	24·6	1·97
	Flacq	30·9	59·6	3·45
	Grand Port	24·5	48·0	3·25
	Savanne	28·7	21·5	1·85
	Black River	39·5	56·7	3·56
	Moka	18·7	4·4	1·23
	Plaines Wilhems	27·0	8·6	1·43
	Average of the above	29·7	32·9	2·42
All Classes	Port Louis	36·6	35·6	2·66
	Pamplemousses	46·1	45·7	3·04
	Riviere du Rampart	39·7	27·6	2·12
	Flacq	42·4	62·7	3·89
	Grand Port	38·6	42·4	2·97
	Savanne	32·5	23·0	1·96
	Black River	44·2	58·2	3·63
	Moka	31·1	11·2	1·52
	Plaines Wilhems	31·9	14·3	1·61
	Average of the above	40·3	35·6	2·60

The exceptionally high figures at an altitude of 1,400 feet (426 meters) were due to a serious local outbreak at the large village of Phoenix in Plaines Wilhelms district, most of which is high and healthy. It should be understood that the higher altitudes in Mauritius are plateaus rather than sloped hillsides, so that the variation is probably due merely to difference of climate. The accepted rule is that the temperature falls 1° F. for every 300 feet (or 1° C. for 164·6 metres) of altitude.

A still larger spleen census was carried out in Ceylon towards the end of 1908 by the orders of the Principal Civil Medical Officer (Sir Allan Perry). He estimated that the total number of children under fifteen years in that island were 1,622,766, and the following table gives the result of the census ("Report of the Advisory Committee for the Tropical Diseases

Research Fund for the year 1909"—Blue Book of Colonial Office).

Province.	Children examined.	Enlargement of spleen.				Spleen Rate.	Average Spleen.
		Total.	Small.	Medium.	Great.		
Northern . .	11,371	5,438	2,567	1,194	777	39·91	2·52
Southern . .	15,022	2,847	1,794	600	453	18·95	1·67
Eastern . .	6,476	2,266	1,449	572	245	34·99	2·19
N. Western .	17,024	9,064	4,675	2,975	1,414	53·25	3·08
N. Central .	5,087	3,215	1,491	1,100	624	63·20	3·64
Uva . . .	5,177	2,291	1,088	686	517	44·20	2·88
Sabaragamuwa	7,154	2,046	1,049	527	470	28·60	2·18
Central . .	14,362	2,804	1,631	817	356	19·52	1·70
Western . .	10,585	2,350	1,867	417	66	22·20	1·59
TOTAL .	92,258	31,421	17,611	8,888	4,922	34·05	2·29

These large figures correspond closely with those obtained in Mauritius. They give not only valuable information regarding the local prevalence of malaria, but show how enormously widespread the disease must be—for neither Ceylon nor Mauritius can be called very extremely malarious. The figures in Ceylon were collected after the malaria season, and those of Mauritius during the early part of the season.

The correspondence between the spleen rate and the number of persons found to contain the parasites will be discussed in section 31 (2).

According to theory the spleen rate should fall with the distance from marshes. This point was carefully worked out by Major Fowler and myself in the outbreak at Phoenix, Mauritius. We found, not only that such a fall occurred, but that it was so extremely rapid and marked as to be beyond all possibility of chance distribution (section 30 (1) and map).

The older writers lay down that the *liver* is generally enlarged in malaria. Kelsch and Kiener [1889] say that its average weight is 2-3 kilograms, but that it may weigh up to

4 kilograms; but they worked in Algeria, and it is quite possible that there, as elsewhere, cases of kala-azar were included. I was struck both in Greece and in Mauritius, where kala-azar is not supposed to exist, with the absence of any marked enlargement of the liver in the great majority of the children who had splenomegaly. More definite data are required on this subject.

23. The Effect of Quinine.—After the earlier physicians had established the utility of cinchona for malaria it began to be much discredited owing to its employment in doses too small to be effective. This error was rectified by Maillot [1834], who after a long struggle persuaded the medical profession to adopt larger doses in consequence of his experience in Algeria. But up to the end of last century, another error, equally great, was very generally made—the drug was not continued long enough to prevent relapses. When I went to India in 1881 it was the rule in military practice to return soldiers to duty three days after the cessation of fever. I believe that I was one of the first to call attention to this absurdity [1896, p. 43]. When I myself was infected next year I prescribed for myself a rigorous quinine treatment for four months, and had no relapses in consequence; and I have always taught this rule since then. The practice is now much better, but I still see numerous patients, especially from West Africa, who have been enjoined to take quinine for a fortnight after recovery! Needless to say, haemoglobinuria is often the result. A third error, still often made, is to withhold the drug until the fever has fallen.

Much work has been done on the *absorption* and *elimination* of quinine by the body — more recently by Kleine [1901], Jacoangeli [1903], Mariani [1903 - 1904], Modigliana [1905], Giemsa and Schaumann [1907], and others. The absorption is measured by the elimination, which takes place principally through the urine, in which the presence of the drug is easily

revealed by chemical analysis; and the results may be summarised sufficiently for public health work as follows:—

After *intravenous injection*, cinchonism follows in a few minutes. Baccelli, who first suggested such injections, employed solutions of 1 gram of quinine in 10 parts of physiological salt solution—to be used only in severe cases.

After being taken by the *mouth*, the first traces of the drug may begin to appear in the urine in about fifteen minutes; but this period varies largely for different salts. The maximum elimination occurs between four to twelve hours after ingestion. Most of the drug is eliminated before thirty-six hours, but traces of it may be found even after nine days. Only about 1/3 of the dose escapes destruction by the body. The more soluble salts appear much more quickly in the urine than the less soluble ones, but the latter are finally absorbed as well as the former. So also, the salts are more quickly absorbed from an empty stomach; but the total amount absorbed from a full stomach is finally quite as large. Given in repeated small doses, the drug appears to be better absorbed than when it is given in a single dose of the same total weight.

After *intramuscular injection*, the salts are not absorbed so quickly, especially if given in strong solution. This is due to local precipitation (Giemsa and Schaumann). Thus while nearly 40/100 of quinine taken by the stomach may be eliminated in three days, only about 12/100 given intramuscularly in a 1/1 solution is eliminated in the same time. But with a 1/10 solution about 23/100 of the salt (bihydrochloride) is eliminated after intramuscular injection.

On the other hand, D. Sandro [1909] favours intramuscular injection because absorption through the liver is thus avoided. He also finds that the drug remains longest in the spleen (twenty hours).

After *rectal injection*, quinine appears in the urine in twenty-five minutes.

As the drug is not completely eliminated for several days it

is cumulative in its action; so that a daily dose will keep the blood impregnated with about twice as much as is given at each dose [from Mariani, 1904]. Fever does not affect the absorption, but gastro-intestinal troubles do so if severe enough.

The following table shows when some of the various salts appear in the urine:—

Salt.	Equivalent of quinine.[1]	Solubility.[1]	Appears in urine.
Bihydrochlorate	72·0%	1/1	15 minutes
Hydrochlorate	81·8%	1/40	15 "
Acetate	84·0%		30 "
Citrate	67·0%	1/820	30 "
Bisulphate	59·1%	1/11	30 "
Sulphate	73·5%	1/800	45 "
Tannate	20·0%	slight	180 "
Euquinine	81·0%	1/12,500	

For further details the laborious papers referred to above, especially those of Mariani and Giemsa and Schaumann, should be consulted.

In large enough doses quinine produces ringing in the ears, deafness, giddiness, headache, dilatation of the pupils, urticaria and erythema, and, in poisonous doses, convulsions, muscular weakness and amblyopia.[2] A very small percentage of people are unable to take even small doses; Dr Macalister had a case in the tropical clinic of the Royal Southern Hospital at Liverpool who could not endure even 0·03 grams.[3] I have observed in experiments on myself that quinine taken just before a meal causes *tinnitus aurium* in about half an hour, thus proving its rapid absorption; but taken after a meal it does not produce this effect so quickly, while it tends to cause much dyspepsia, especially if given in pill form, and leaves a

[1] Very variable figures are given. I adopt some of those furnished to me by Howards & Sons.

[2] G. Baermann has recently reported [1909] a death due to two doses of 1/2 gram of quinine hydrochlorate—collapse, blood in dejecta, extravasation in organs.

[3] I have just seen a patient who confessed to the rather exceptional drug-habit of quinine. He acquired a craving for it, and found great difficulty in breaking himself of the habit.

taste in the mouth which remains until the next meal. For myself, therefore, I always prefer to take it just before meals, especially before breakfast. This is also often the best time, according to Torti's law, for destroying the plasmodia.

The action of quinine upon the parasites has been studied by many observers. Binz [1867] was the first to study the effect of weak solutions of quinine on Protozoa. Laveran states that a 1/10,000 solution kills the plasmodia *in vitro*. Romanowsky and Mannaberg [1894] found that in patients who had been taking quinine the parasites possessed badly staining nuclei, that many of the spores within the sporocysts were dead, and that the understained parasites had a peculiar appearance. Numerous studies have been made subsequently on the subject, as by Ziemann [1906] and Craig [1906], which give a somewhat different picture of the death-process. Nearly all observers have agreed that quinine exerts much less influence on the gametids, especially the crescents, than upon the sporids, though Ziemann thinks that the male gametids are more easily affected. Many of the younger writers ascribe relapses to parthenogenesis in gametids which survive all quinine treatment in this manner. Gualdi and Martirano [1901] state that as much as 2·5 grams of quinine in single dose, or 1·0-1·5 grams daily, are not sufficient to banish crescents. I note that it is not always easy to state that the observed forms are really due to quinine and not to the germicidal action of the blood; and indeed that they are really abnormal forms at all; and also that strict enumerative methods have not been used.

An important point in connection with the prevention of malaria is that Bignami and Bastianelli, Gualdo and Martirano, and Schaudinn [1903] have succeeded in infecting Anophelines from patients who had been taking quinine. Schaudinn's case had been taking 1 gram thrice a week. Ziemann remarks that this suggests that the male gametids are not destroyed as he thought. It also suggests that cases of crescents are likely to prove sources of infection in spite of energetic treatment.

The article of Caccini [1902], which I quoted in part in section 20, gives valuable information on the effect of quinine treatment on the prevention of relapses, and therefore on the extermination of the invasion. After all, this is the most important part of the subject from the point of view of preventive medicine. He summarises his cases of benign tertian as follows:—

(1). Of 145 cases treated early and systematically a relapse occurred in 37%.

(2). Of 301 cases treated systematically, but started somewhat late, a relapse occurred in 30%.

(3). Of 1,002 cases with daily quinine, a relapse occurred in 15%.

(4). Of 50 cases treated with quinine at height of fever, a relapse occurred in 80%.

(5). Of 55 cases treated with quinine during the sweating stage, a relapse occurred in 80%.

(6). Of 291 cases to which quinine was given irregularly, a relapse occurred in 85%.

(7). Of 120 cases to which quinine was not given at all, there was only one in which no relapse occurred.

The author explains that by "early and systematic treatment" he means quinine given on alternate days in doses of 1·5-2·0 grams per day in three or four doses, with about half an hour's interval between them, starting the administration three hours before the (expected) commencement of the fever. *But the treatment was suspended after seven days.* By "systematic but late treatment" he means the same scale of treatment commenced later in the case. By "daily quinine" he means daily administration of 1-2 grams without reference to the hour of onset of fever, the treatment being *suspended after seven to ten days.* Apparently in all cases "the cure was recommenced every time that a relapse set in."

The figures prove fairly clearly the following:—

(1). The best results (only 15% relapses) were obtained with daily doses of quinine.

(2). Good results (only 30-37% relapses) were obtained with the so-called systematic treatment.

(3). Bad results (80-85% relapses) were obtained by the use of quinine late in the paroxysm.

(4). The unfortunate (? unavoidable) suspension of the treatment after seven to ten days has invalidated most of the author's results regarding the length of the apyrexial period, since, obviously, the parasites would begin to multiply again from the moment that the quinine was stopped and would produce another attack when, a week or so later, they had multiplied sufficiently to do so. It is true that he says in his summary that "though the treatment is prolonged beyond the seven days the relapse occurs all the same; but this was done apparently only in twenty cases altogether (so far as I can follow the intricacies of the paper), and, as every one in practice knows, to order a medicine is one thing, to get it taken is another.

With regard to quartan fever there is nothing to add. With regard to malignant fever the author is very emphatic on the point that relapses occur between the fifth and twenty-first days of apyrexia, in spite of energetic quinine treatment; for example, in the case of 90 work-people discharged from hospital after temporary cure, and 792 patients kept in hospital for thirty days, all the former and all except 24 of the latter relapsed. All that the author is able to admit is that the relapses occurred sooner in those discharged from hospital than in those retained there. Unfortunately, he is quite vague as to the continuity of the treatment. In one place he says that "this of course, occurred in patients who had continued the quinine treatment in doses of 1-2 grams per day," and in another place that this had been done "even after the cessation of the febrile attacks," but I do not clearly gather that the treatment had been *sufficient and continuous* through-

out. Obviously the whole investigation, meritorious as it was, has lost much of its value owing to the uncertainty introduced by the discontinuous methods of quinine administration employed. Regarding relapses after "long intervals" the same element of doubt exists. I suppose that as long as a single sporid remains alive in the blood, the parasites may again reach the pyrogenous limit after a sufficient interval, if the drug is discontinued or continued in insufficient doses. A very careful enquiry is demanded before we can admit with the author that quinine has no effect on the occurrence of such relapses.

Subsequently A. Carducci [1905] made a similar analysis of 50-60 cases at Rome. He infers that relapses at short intervals occur from the 6th-9th days of apyrexia, mostly on the 7th day, but that in quartan and tertian a relapse is often passed over. He recommends 1 gram of quinine on the 1st, 2nd, 5th, 6th, 8th, 13th, 15th days of apyrexia, and 1·5 gram on the 7th and 14th days in order to check this weekly return, and so on for months and months. With this medication he was able to prevent relapses in most of his cases, but remarks that they recurred if the quinine was discontinued.

On the whole, after studying this and much other literature, I think that there is probably no special periodicity in relapses —that the periodicity noted by some observers was probably due rather to their own periodical methods of medication than to any other cause. If marked periodicity in relapses exists, it should have been widely observed long before now amongst the immense number of cases occurring in military practice— it would certainly have been observed by the military as well as by the medical officers. But I can find few notes to this effect, and certainly remember nothing of the kind in my own practice. As stated in section 20 (6) I am at present inclined to think that relapses occur after quite irregular intervals, determined by secondary causes or insufficient treatment, rather

than by any inherent property of the parasites; but that there may be some tendency to periodical failure of the germicidal powers (section 65).

Regarding the reputed failure of quinine to prevent relapses, I again remain dissatisfied with the evidence. It appears to me to be merely a question of sufficiency and continuity of treatment.

On the other hand, there appears to be a large mass of evidence in favour of Torti's law that quinine should be administered two or three hours before the commencement of the paroxysm, if this can be determined.

Many authors—Laveran, Koch, Manson, Ziemann, Carducci and others — have suggested various systems of discontinuous dosage, such as the administration of large doses once a week, or twice a week, or every four days, or for two consecutive days every week, or every seven days after the first day of apyrexia, and so on. Owing to the temporary retention of the drug in the blood there is difficulty in understanding the *rationale* of such proposals. Moreover, a large dose every few days is much more distressing to the patient than a smaller dose every day, and, for this very reason, is apt to be neglected or postponed by him. There appear to be no evidence that the plasmodia become habituated to quinine as trypanosomes do to atoxyl. Personally, therefore, I prefer the continuous method; that is, a dose at least once a day. It seems to have given the best results in Caccini's cases, and is recommended by Celli. We should here distinguish carefully between what may be called *medical* and *hygienic therapeutics.* The former is the therapeutics of the physician at the bedside; the latter, that which is to be recommended to the general public—who are, in fact, called upon to treat themselves. Refinements in medication which are possible in the former case are not possible in the latter one, and therefore the simplest possible rule is the one which is to be preferred for hygienic therapeutics (section 36).

Regarding the duration of treatment required to exterminate the parasites, we have no complete data. The case of T. Manson, who was infected in London in 1900 and was treated for three months, relapsed after nine months. In India the after-treatment of the European troops is continued for six weeks after discharge from hospital, resulting in a considerable reduction in the number of admissions. Thus out of ninety primary cases in two stations there were only seven relapses ("Annual Report of Sanitary Commissioner, 1907," p. 11). Teaching on this point differs largely, but I have always given four months as the *minimum* period, and doubt whether even this is quite sufficient. No harm is done if the patient continues some quinine for six months.

With regard to the different preparations of quinine, much more strict work by enumerative methods on the effect of the various salts on the parasites is required. Estimations of the amounts excreted in the urine and so on give but partial information on the germicidal action—which is the point of importance.

The more soluble salts—bisulphate, bihydrochloride, hydrobromide—are most easily absorbed, and appear most quickly in the urine.

The less soluble salts—sulphate, citrate, tannate, euquinine—are absorbed much more slowly, but, finally, in just as much quantity.

Exact enumerative experiments are therefore required to decide which of these groups of salts have the most germicidal action.

Much has been said recently in favour of the tannate, *e.g.* by Nardelli [1909] and Deaderick [1909].[1] It seems to be slowly but largely absorbed; it remains in the blood; it is inexpensive, nearly tasteless and very suitable for prophylactic use by children (section 36).

Methylene Blue was suggested for malaria by Guttmann

[1] And Celli [1910].

and Ehrlich [1891], and has been studied by Thayer [1892], De Blasi [1902], Wood [1905], Ruge [1906] and others. Atoxyl has been recently tried by Koch, Grosch [1907] and various Greek observers. The general conclusion is that it does reduce the sporids to some extent, but not so well as quinine. The investigations must be revised and extended. The methylene blue must be pure, and should be given in coated pills or capsules up to 1 gram or more a day; the atoxyl may be given by injection up to 0·2 grams twice a week. These and other drugs are not yet of importance for preventive purposes.

24. Summary.—The doctrine of the parasite invasion given above may be summarised as follows:—

The infecting Anopheline probably injects at a single feeding from a few to several thousand protospores of one or more species of *Plasmodium*.

Many of the injected protospores probably perish; but the survivors enter haematids and begin to multiply at an average rate depending upon their species and, possibly, certain other factors.

When the number of parasites reaches something like 50 per *c.mm.* of blood the patient begins for the first time to have definite symptoms of illness.

About the same time some kind of germicidal power which opposes the invasion begins to come into play, and tends, in the great majority of cases, to limit the number of parasites below a fatal number.

At the same time some increasing antitoxic power tends to reduce their effect upon the host.

The further history of the invasion is probably that of a constant struggle between the parasites and the germicidal power, tending, in the great majority of the cases, to the final victory of the latter.

In untreated or non-immune cases, this struggle generally

produces a long series of relapses, which tend to be precipitated by many secondary influences acting on the host.

Finally, after this series of rallies and relapses the patient appears often to reach a stage of partial (or ? complete) immunity.

Possibly, if reinfection does not take place, something like half the cases tend to recover spontaneously every three months.

The effect of reinfections upon the severity or duration of the case has not been estimated.

The case may be cut short at any time by death, spontaneous recovery, or quinine.

There is no reason why we should suppose that the infection is maintained by encystment or parthenogenesis or other special arrangements of the parasites, and the existence of such phenomena has not been proved.

This doctrine can scarcely be verified by the continuous study of entire cases, and is therefore based rather on the comparison of different periods in the history of many different cases, and on reasoning from numerous data.[1]

The careful student of the literature of the pathology of malaria will be struck by the immense amount of admirable work done on the subject. Ideas of number and quantity, however, are often completely disregarded—a common fault of biological work—with the result that many of the secondary theories and explanations will require to be examined over again by more laborious, enumerative and quantitative methods.[2]

[1] I have held it, in the absence of a better doctrine, since 1895, and have always taught it in my lectures and publications.

[2] See section 65.

CHAPTER V

MALARIA IN THE COMMUNITY

25. Is the Infection caused otherwise than by Anophelines.—Having considered the parasitic invasion of the individual, we must next examine that of the community.

It was shown in sections 16 and 17 that infection is produced by the bite of certain mosquitos; but after proof of this was obtained, several capable students of malaria expressed a doubt as to whether this is the only route of infection. We can scarcely wonder, then, if the general public sometimes have similar doubts, and the Health Officer must be always prepared to discuss the matter fully in public. The following questions are easily answered :—

(1). *Do mosquitos exist where there is no malaria?*—They do; but this has no application to the subject, as only certain species carry malaria.

(2). *Does malarial fever occur where there are no mosquitos?*—Very frequently, as on board ship in mid-ocean, on high mountains, or in Britain in the middle of winter; but such cases are always relapses.

(3). *Do Anophelines exist where there is no endemic malaria?*—In many places, as in Britain; but the presence of infected persons, as well as of carriers, is required, and, moreover, both must be in *sufficient numbers* (section 28).

(4). *Does endemic malaria occur where there are no Anophelines?*—No case of this has yet been established. So far as we know, Anophelines exist in all warm countries, and in most temperate climates during the summer.

But the Anophelines are often difficult to find. Thus, I had been working on the mosquito theory of malaria for two years before I noticed this group of gnats. Duggan [1897] said that there were few mosquitos in Freetown, Sierra Leone, but we found numbers there two years later. In 1901 I observed none flying about in a new house at Ibadan, Lagos, West Africa; but five or six, mostly Anophelines, were captured two nights in succession within an old mosquito-net with holes in it, in which a servant slept. If so many had been able to enter through the holes during the night, how many (though none were observed) must have been flying about free? They abound in Britain, though the general public scarcely ever notices them. Before the presence of Anophelines in any locality can be denied, a trained observer must be employed to search for them. Many workers have made similar notes on this point.

It does not follow, because the insects are scarce in a locality at the moment when we visit it, that they do not abound in it at some other season.

(5). *Is it possible to become infected in uninhabited localities?* —Some people, such as several sportsmen and travellers I have met, have stated that they acquired malaria in such places. One told me that he had frequently been attacked a few hours after entering a certain big-game district. We ask in reply (*a*) what about the incubation period; (*b*) whether the traveller was unaccompanied by servants; and (*c*) whether he had travelled in a balloon? We must obviously pass through many inhabited places, where the risks of infection may be great, before reaching the wilderness; we must have servants and carriers with us; and we may indeed have had the parasites in our own blood, without knowing it.

(6). *If mosquitos become infected from men and men from mosquitos, how and when did the process first commence?*—This question is always asked by the most intelligent person present; and I only wish that we knew the answer. It is concerned

with the whole subject of the evolution of metaxenous parasites. Leukart supposes, of course, that they were once free living organisms, which acquired the habit of living first in one host and then, for greater security during the necessary period of migration, in another; and Grassi, and A. J. Chalmers [1902] have tried to fit this hypothesis to the case of malaria. But there are certain grave difficulties, and for the present it is sufficient to note that we must be satisfied with the facts as we find them.

(7). *Has it been absolutely proved that malaria does not come from the soil?*—It is always very difficult to *prove* a negative hypothesis of this kind. We never know what surprise Nature does not keep in store for us; we are not omniscient, and wonderful things happen. But the fact that we cannot prove that malaria does not come from the soil is no proof of the hypothesis that it does do so. We are not sure that elephants do not exist in the moon; but our ignorance here does not prove that they do exist there. Malaria may just possibly rise from the soil; but there is no evidence at all that it does so. In my experience, those who argue in favour of this speculation nearly always confound relapses with original infections. True, we occasionally meet with cases which are not easy to explain on the mosquito theory, but I have never met one which could *not possibly* be explained by it.

On the other hand, there are many strong, very strong, arguments against the idea that malaria is bred directly in the soil or in the marsh. In the first place, all experiments to infect men with air or water brought from malarious localities have failed (section 17). Secondly, if malaria were due to any particular kind of soil under certain conditions (as has been supposed), it should always be present where that soil and those conditions exist. But we know that actually it comes and goes. For example, it came to Mauritius in 1866, and to Réunion in 1867; it is still absent from Seychelles and Rodrigues, though favourable conditions for it exist in those

islands, and it has disappeared from Great Britain. But the soil and the climate of these areas have not changed. Thirdly, if the poison is diffused in the air it ought to affect every one within a considerable area round the generating centre, but as a rule the disease is limited to the immediate vicinity of the marsh. Fourthly, the telluric miasm ought to attack especially those who are engaged in digging, but I have never observed that cultivators and gardeners suffer much more than their neighbours; while, as a matter of fact, it is generally the children and even the infants who suffer the most. Lastly, the idea that the parasites can live in soil, water and air, as well as in men and mosquitos, is extremely improbable in the light of our general knowledge of parasites.

The last is probably the clinching argument. Living organisms do not possess independent properties, but accord more or less in their structure, capacities, habits and life-history with other organisms. We are cognisant of thousands of parasites of men, animals and plants; and what we know of the parasites of malaria shows that they are not markedly exceptional. In fact they belong to a class of parasites which infect two hosts, one of which feeds on the other—as, for instance, parasites of the deer and the tiger, the mouse and the cat, the ox and man, the ox and the cattle tick, and now man and mosquitos. The general law is, therefore, satisfied by the known life-history of malaria. We have no reason to expect another life-history for the malaria parasites in soil, water or air, any more than for the other parasites. Then again, every animal possesses only the limited powers which have been given to it by the evolution of ages, and for which it has acquired definite organs and habits of life. The mole burrows, the fish swims, the bird flies, the parasite occupies the higher animal or plant. But if this telluric hypothesis is sound, what remarkable animals must these parasites of malaria be! They already possess a structure wonderfully adapted for their life in men and also in mosquitos; but we must now

expect that they are also able to burrow like the mole, swim like the fish, and fly like the bird! To do all these things they must have the suitable organs; and not only this, but they or their spores must be protected against heat and cold and hosts of enemies in soil, water and air. If all this were true we should have to put the parasites of malaria in a special class by themselves, apart from the rest of creation.

These reasons have now led pathologists and parasitologists (who can perhaps appreciate their weight better than others) to abandon the telluric hypothesis as a likely one. There is no evidence in favour of it, and there are very strong arguments against it. The true theorem is obvious. The connection between malaria and the marsh, so long known to suffering humanity, is now fully explained by the fact that the Anophelines breed in the marsh. There is no necessity to believe that the germs also breed in the marsh. Malaria comes from the marsh, not because the germs of the disease come from it, but because the carriers of the germs do so. It is the same thing in the end. The ancient theory was quite right. Malaria is caused by a marsh miasm. The Anophelines themselves, the mosquitos, *are* the marsh miasm.

(8). *Do other insects besides Anophelines carry malaria?*—First we should note that not all, but only certain species of Anophelines carry it, and that according to some, only certain varieties of some of these species are effective. As a fairly general rule, animal parasites are very particular in their choice of hosts. Thus no one has succeeded in infecting animals with human malaria, and the probability is that it will not exist in many kinds of mosquitos. Between 1895 and 1899 I failed entirely in infecting several species of *Culex* and *Stegomyia*, though I made experiments on hundreds of the insects; and these results were confirmed by the Italian observers, by Stephens and Christophers, and by many others. But, nevertheless, such negative results are never absolutely conclusive unless enormous numbers of experiments are made,

because it is always possible that some condition such as of temperature or humidity may have been overlooked, or that the proper species or variety may not have been used. But there is a strong argument against mosquitos in general being concerned, namely, that from the oldest times malaria has been known to be connected with marshes, while many mosquitos, such as most *Culex* and *Stegomyia*, do not breed in marshes as a rule, but in petty collections of water round houses. If these carry malaria, then malaria should abound everywhere, especially in towns, and not so exclusively near marshes. A good contrast is found in the case of yellow fever, which does abound in towns and not particularly near marshes —for the simple reason that it is carried by *Stegomyia.*

On the whole, then, though we cannot say definitely that malaria is not carried by other hosts than Anophelines, yet there are strong reasons for this opinion. At all events we are fairly certain that most insects which carry it must be marsh-born, like the Anophelines; so that the principal preventive measure of drainage is not seriously affected by the question.

(9). *Do the human parasites live also in animals?*—In 1898 in Calcutta, my servant, Mahomed Bux, became suddenly infected with malaria, which he thought he had acquired while collecting mosquitos fed on infected birds. At present, however, no one thinks that the parasites of men and of animals are the same. The attempts of Di Mattei and others to infect birds from men were all negative.

(10). *Do the parasites enter the eggs and larvae of mosquitos?*—In 1898 also, I thought that the protospores of *P. danilevskyi* might enter the eggs of the carrying mosquitos, but all my attempts to find them there were failures. More recently Schaudinn [1904] has sought to revive this hypothesis; but he gave no proof of it. Such a thing happens, however, with *Piroplasma* in ticks, and has been suggested as possible in the case of yellow-fever. If it happens in malaria, the parasites would be passed on, not only from mosquito to man, but also

from mosquito to mosquito. I have had no opportunities for examining the subject further, but may add that my studies on the point in 1898 were very careful. We should not accept the suggestion, even tentatively, without much better evidence.

26. Some Definitions.—We are therefore almost sure that infection with the parasites of malaria occurs in nature only through the bites of mosquitos — probably only of certain species of Anophelines. But this statement is not nearly sufficient for all the purposes of prevention, and we must discuss the subject in much greater detail. Why does the disease vary in amount from place to place, or even in the same place from season to season, or year to year? Why does it disappear from some countries and appear in others? The whole subject of the prevention of malaria is based upon a rational study of these questions, and the reader who is called upon to undertake such work must exert himself to consider them thoroughly. We must first have clear ideas upon the following points:—

(1). The cases of malarial fever which occur within a given locality belong, of course, to two classes, the *indigenous cases* and the *imported cases*. The first are those who were infected within the locality; the latter, those who were infected outside it, but who, after immigration, remain infected within it. The latter class may be very numerous in military stations, hill stations, and sanatoria. But they also abound in many villages of which the inhabitants are given to working elsewhere during the malarious months; and among bands of immigrant workmen. On the other hand, in small towns and villages with fixed populations most of the cases are apt to be indigenous ones. It is often difficult to determine the proportion of each class because many of the imported cases may be reinfected within the area

of observation, while many of the indigenous cases may have emigrated since infection.

(2). By the *malaria rate* of a locality I mean the percentage of persons who contain plasmodia at some given moment. We may divide this into the *indigenous malaria rate* and the *imported malaria rate.* The former may also be called the malarial *endemicity* of the place.

(3). By the *malaria index*, or *endemic index*, of a locality I mean the percentage of persons in whom any *evidence* of malarial infection was *found* at some given moment. It will be clear from Chapter IV that such evidence cannot generally be found in all the cases of malaria in a locality—many may contain plasmodia without showing them or any objective symptoms of them. Conversely, many people may show evidence of past infection, such as enlarged spleen, without containing plasmodia at the moment. We may divide the endemic (or malaria) index into *the adult*, *the juvenile* and *the infantile endemic index.*[1]

(4). By the daily, monthly or annual *inoculation rate* of a locality I mean the percentage of persons who were inoculated or reinoculated by mosquitos during the day, month or year referred to.

(5). When we talk of the *amount of malarial fever* in a locality we generally refer to the total malaria rate in it. When we talk of the *amount of malaria* in it we generally mean its indigenous malaria rate. When we say that a place or a season is very *malarious*, we mean that the inoculation rate is very high for that place or season.

[1] The term "endemic index" was first used by Stephens and Christophers to denote the percentage of children showing the parasites in the blood, but the above definition is more correct.

(6). By the *malarial case mortality* we mean, of course, the proportion of infected persons who die of the disease.

(7). By the *malarial mortality* we mean the proportion of the total population who die of the disease.

(8). I use the word *ratio* to denote a proportion, and the word *rate* to denote a percentage. The latter is obtained by multiplying the former by one hundred.

27. Conditions required for the Production of New Infections in a Locality.—From what has been written already it will appear that new infections can occur in a locality only if all the following conditions exist:—

(1). That a person whose blood contains a sufficient number of gametids (sexual forms) is living in or near the locality.

(2). That an Anopheline capable of carrying the parasites sucks enough of that person's blood.

(3). That this Anopheline lives for a week or more afterwards under suitable conditions—long enough to allow the parasites to mature in it.

(4). That it next succeeds in biting another person who is not immune against the disease or is not protected by quinine.

To these we must now add the following principle:—

(5). That few or no new infections will occur in a community unless the persons with gametids in their blood and the carrying Anophelines are sufficiently numerous.

This last proposition is the basis of the public prevention of malaria. Although tacitly admitted, it is never properly discussed in the monographs, and the reader must therefore examine it here in detail and as precisely as possible.

Let us suppose that we have to do with a population of 1,000 people living over an area in which indigenous malaria does not exist; and suppose that one of these people is an

imported case with suitable gametids in his blood. Next, suppose that a single suitable Anopheline is liberated within the area. What are the chances that this insect will ever cause a new infection?

First, we observe that not every mosquito can succeed in biting human beings at all: suppose that the chances are 3 to 1 against this happening. Next, we observe that as there is only one infected person among the 1,000 people in the place, and as the particular Anopheline liberated in the area may bite any one of these people, the chances are 999 to 1 against its happening to bite the patient, even if it succeeds in biting at all. That is, altogether, the chances against its biting the patient are 3,999 to 1. But suppose that this has happened. It must now live for a week or more afterwards, and not all mosquitos live so long. Suppose that the chances are 2 to 1 against this particular Anopheline living long enough to mature the parasites in it—so that the chances are 11,999 to 1 against the Anopheline reaching the infective stage. But it must now bite a second person. Suppose that the chances against this are, again, 3 to 1. Thus the total chances against this Anopheline inoculating another person will be something like 48,000 to 1. In other words, if instead of liberating a single Anopheline within the area of observation we had liberated 48,000, the chances are that only one of these would succeed in biting the patient, and would also live long enough to become infective and to bite a second person. Of course, by bad luck, so to speak, a larger proportion of the insects might succeed in this; but, on the other hand, by good luck, none at all might succeed. Such calculations, though obviously based on conjectural data, yet suffice to show the absurdity of supposing that the presence of a few Anophelines must cause an epidemic of malaria.

Or we may consider the subject as follows:—Suppose that 48 Anophelines on the average live near the infected person. Then, if the conjectural data are sound, 1/4 of these, or 12,

will succeed in biting him ; 1/3 of these, or 4, will live for a week or more ; and only 1/4 of these, or 1, will succeed in biting another person. If 48 Anophelines live near each one of the 999 healthy people, there will be 47,952 Anophelines which can bite only healthy people, and which cannot therefore cause new infections. Thus, on the average, only 1 out of 48,000 Anophelines will succeed in infecting another individual.

If instead of only one person with gametids in the blood there are 2, 3, 4 . . . or more in the locality, then if there are 48 Anophelines on the average to each person, 2, 3, 4 . . . or more new infections may be caused. Thus, obviously, the number of new infections in a locality, that is, the *inoculation rate*, depends on two factors (other things being equal), namely, the number of Anophelines and the number of previously infected persons in the locality.

It is useful to put these thoughts into simple symbolic language.[1] Let p denote the human population of the locality ; mp the number of malaria-infected persons ; and imp the number of these with gametids in the blood. Here m and i are fractions, since mp is less than p, and imp less than mp. Also m may vary from 0 to 1, since the number of infected persons may be anything from zero to the whole population. The fraction i, being the proportion of infected persons with gametids in their blood at the moment of enquiry, may be put at 1/4, or may be much less.

Again, let a denote the number of Anophelines (of some malaria-bearing species) to each human being — so that ap denotes the total number of Anophelines in the locality, and $aimp$ the number of Anophelines compared with the number of persons with gametids. Let b be the proportion of these (say 1/4) which succeed in biting ; s the proportion (say 1/3) which succeed in maturing the parasites ; and b the proportion which succeed in biting another person. Then $bsbaimp$ gives the number of Anophelines which succeed in infecting persons.

[1] For fuller treatment see section 66.

We have assumed that $b=1/4$, $s=1/3$, $i=1/4$; so that if each of these bites a different person, we shall have—

$$\text{No. of inoculations} = b^2saimp = amp/192$$

$$\text{Inoculation rate } \% = \frac{amp}{192} \cdot \frac{100}{p} = \text{about } \tfrac{1}{2}\, am.$$

That is to say, the inoculation rate per 100 of population equals about half the malaria ratio (m) multiplied by the mosquito ratio (a).

For example, in a village containing 1,250 people, 750 infected people, and 3,000 Anophelines, $p=1{,}250$, $m=0{\cdot}6$, $a=2{\cdot}4$; and we calculate roughly that the number of infecting Anophelines, and also of inoculations, will be about 9·4. The inoculation rate per cent. will be about $1/2\ (2{\cdot}4 \times 0{\cdot}6) = 0{\cdot}72$; that is, the chances of being inoculated in the village will be as 72 is to 10,000.

Or suppose that in another village half the people are infected, and there are about twenty Anophelines to each person. Then the chances of becoming infected there will be about 5/100.

Such calculations may appear far-fetched to many; but they are useful, not so much for the numerical estimates yielded by them, but because they give more precision to our ideas, and a guide for future investigations.

28. Laws which Regulate the Amount of Malaria in a Locality.—The number of infecting mosquitos which succeed in biting again is b^2saimp. If all of these bite different people, and all these people are healthy, and all become infected, this expression will also denote the number of *new infections* occurring in the locality. But, of course, the infecting mosquitos may often happen to bite on the second occasion, not healthy persons, but persons already infected, especially if the proportion of the latter is large.

If, as before, mp denotes the number of persons already infected at the beginning of the enquiry, then $p-mp$ or $(1-m)p$ denotes the number of healthy people. Hence by proportion

the number of infecting mosquitos which bite healthy persons (on the second biting) will be x, where

$$x : b^2saimp :: (1-m)\,p : p$$

or $$x = b^2sai\,(1-m)\,mp \quad . \quad . \quad . \quad . \qquad (1)$$

and if each bites a different person and each person becomes infected, the same expression will denote the number of *new infections* which occur in the locality—that is, will denote the addition to the number of malaria cases there.

But this is not the whole change which may occur. While, during the period of observation, new infections are being produced, it may happen that some of the old cases may have recovered. The number of those old cases was originally mp: let rmp denote the number of those who have recovered during the period of observation—so that r is a fraction. Hence the whole number of cases in the locality will have increased or decreased at the end of the period of observation, according to whether $b^2sai\,(1-m)\,mp$, the number of new cases, is greater or less than rmp, the number of recoveries. Thus (neglecting common factors) the change depends upon whether $b^2sai\,(1-m)$ is greater or less than r.

Suppose that no change occurs—that the recoveries exactly equal the new infections. Then

$$b^2sai\,(1-m) = r \quad . \quad . \quad . \quad . \qquad (2)$$

From this equation we can calculate the values of a and m when the amount of malaria in the locality remains constant—that is, if we know the values of b, s, i, and r.

Suppose that the period of observation is one month. Now, in section 21, I estimated roughly that only about half the cases of malaria remain infected after three months. If this rate holds for smaller periods, we may suppose that the ratio of people who remain infected after only one month will be given by the cube root of 1/2—that is, by 0·7937; so that the proportion of those who recover in one month will be $1-0·7937$. That is, we may write $r=0·2063$. Let us take the values of b, s, i as suggested in the previous section—so that $b^2si = 1/192$.

Hence the new infections will equal the recoveries if

$$a\,(1-m) = 192 \times 0{\cdot}2063 = 39{\cdot}59 = 40\ (say) \qquad . \quad . \quad . \qquad (3)$$

and the malaria will increase or diminish in the locality according to whether $a\,(1-m)$ is greater or less than 40. Thus, if m, the original malaria rate, is very small, the malaria will not increase unless a, the monthly number of Anophelines per head of human population, is greater than 40. On the other hand, if m is a larger fraction, say 1/2, the malaria will not increase unless a is greater than 80. If m is still larger, say 3/4, the malaria will not increase unless a is greater than 160. This suggests that the malaria rate is not likely to increase indefinitely unless the number of mosquitos is enormous. On the other hand, if the number of Anophelines is below the figure given by the equation, the malaria rate ought to begin to fall, *because the new infections can no longer keep pace with the recoveries.*

It should be noted that by the number of Anophelines we here mean the number of *different* Anophelines which may bite each person *during a whole month*, and not the insects which may be, so to speak, allotted to each person at any one moment. We say one month because this is the period we have selected for observation — the period during which we suppose that 0·2 or 1/5th of the cases recover. If we had selected one week for the period of observation, the proportion of recoveries would, of course, be lower (about 0·056142), and the number of different Anophelines to each person, required to compensate for the recoveries during the week, would be correspondingly less (as shown by the equation).

Now, what will happen if the malaria rate, instead of remaining constant, increases or decreases? On the one hand, will it increase until every one becomes infected; or, on the other hand, will it decrease until it vanishes?

We have supposed that mp denotes the number of cases at the beginning of the enquiry, which lasts, let us say, for a month. Let m_1p, m_2p, m_3p . . . denote the number of cases

at the end of 1, 2, 3 . . . months respectively. Then if the other figures remain constant, we have,

$$m_1p = original\ cases + new\ infections - recoveries$$
$$= mp + b^2sai\,(1-m)\,mp - rmp \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

and as the same process repeats itself month after month we continue to have,

$$m_2p = m_1p + b^2sai\,(1-m_1)\,m_1p - rm_1p$$
$$m_3p = m_2p + b^2sai\,(1-m_2)\,m_2p - rm_2p$$
$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad .$$

and so on. We can calculate m_1 from the first equation. Substituting its value in the second equation we calculate m_2; and substituting this in the third equation we calculate m_3; and so on indefinitely.

If $r = 0{\cdot}2$ and $b^2si = {\cdot}005$, the value of the *nth* term of the series $m_1p, m_2p \ldots m_{n-1}p, m_np$ may be written

$$m_np = (160 + a - am_{n-1})\,m_{n-1}\,p \times 0{\cdot}005 \quad . \quad . \quad . \qquad (5)$$

Let us now consider for examples the case of a village containing 1,000 people of whom half are infected at the beginning of the enquiry. By equation (3) of this section, and also by this equation, if the number of different Anophelines *per* person during one month (a) is 80, the malaria will neither increase nor decrease, so that m_n will always be the same as m, namely, 0·5. But if a is greater or less than this, the malaria will increase or decrease accordingly.

(1). First, suppose that there are 100 different Anophelines *per* person during a month—so that the malaria should increase. Thus we calculate,

$$m_1p = (160 + 100 - 100 \times 0{\cdot}5) \times 5 \times 0{\cdot}5 = 525$$
$$m_2p = (160 + 100 - 100 \times 0{\cdot}525) \times 5 \times 0{\cdot}525 = 544{\cdot}5.$$

Proceeding in this way we find that the number of cases should increase every month as follows:—

Increase of Cases with 100 *Anophelines*

Months	0	1	2	3	4	.	.	.	*finally*
Cases	500	525	544	560	571	.	.	.	600

But how do we reach the last figure? As we calculate the number of cases month after month we observe that they always increase, but by a constantly decreasing increment. Finally, this increment becomes very small, so that the number of cases *approaches a fixed limit.* It is easy to calculate this limit mathematically (as will be done presently), but we can also calculate it very simply as follows. If the number of cases does ever arrive at a fixed limit, so that it no longer increases, then equation (3) of this section must hold; that is, the number of cases and the number of mosquitos will be exactly balanced according to the formula $a\,(1-m)=40$. From this we have, when this exact balance is reached,

$$m=1-40/a \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

Here $a=100$; so that finally $m=1-0{\cdot}4=0{\cdot}6$; which, when multiplied by the population (1,000), gives 600 cases as the final limit.

(2). Next, suppose that there are only 60 Anophelines *per* person. Then we calculate as before,

Decrease of Cases with 60 *Anophelines*

Months	0	1	2	3	4	. . .	*finally*
Cases	500	475	455	438	424	. . .	333

The final result is obtained as before from equation (6). Here then, although the malaria ratio diminishes rapidly at first, it never disappears altogether, but ultimately stands at 1/3 of the population.

(3). Next suppose that there are 40 Anophelines *per* person.

Decrease of Cases with 40 *Anophelines*

Months	0	1	2	3	4	. . .	*finally*
Cases	500	450	409	376	348	. . .	0

Here the fall is quicker and the malaria finally vanishes, because $m=1-40/40=0$; but very many months will elapse before this result is approached.

(4). We also have

Decrease of Cases with 1 *Anopheline per head.*

Months	0	1	2	3	4	. . .	*finally*
Cases	500	401	322	259	207	. . .	0

(5) *Decrease of Cases with no Anophelines.*

Months	0	1	2	3	4	. . .	*finally*
Cases	500	400	320	256	205	. . .	0

Thus the presence of only a small number of Anophelines does not affect the result very much. In fact we can see this from the form of the equation (5), namely,

$$m_n p = \{160 + a(1 - m_{n-1})\} m_{n-1} p \times 0{\cdot}005,$$

because $1 - m_{n-1}$ is always a fraction, and a must therefore be a considerable number if it is to have any marked effect compared with the first term 160.

Returning now to the subject of the *limit* mentioned in example (1). It is known in mathematics that if a series such as m_1, m_2, m_3 . . . here considered tends to a limit, that limit can often be easily found. Let m denote the limit—that is, $m = m_n$ when n, the number of terms, is indefinitely increased. In other words, m is the malaria ratio which is *finally* arrived at after the lapse of many months.

Now we had in equation (4),

$$m_n p = \{1 + b^2 sai\,(1 - m_{n-1}) - r\} m_{n-1} p$$

By the mathematical rule referred to, the value of m_n, or m. can be easily found, when n is large, by solving the equation

$$mp = \{1 + b^2 sai\,(1 - m) - r\} mp$$

that is,

$$m = 1 - r/b^2 sai \qquad . \quad . \quad . \quad . \quad . \qquad (7)$$

If $r = 0{\cdot}2$ and $b^2 si = 1/200$, this gives $m = 1 - 40/a$, which is the same as equations (2) and (3), which have already been used to find the limit shown in example (1). There we calculated the limit by another line of reasoning, but now we see that it can be obtained also by the ordinary mathematical rule. (This rule is that if $\phi^n x$ is a repeated function of x, its value when n

is indefinitely large will often be given by the roots of the equation $x = \phi x$).

The reader should make a careful study of those ideas, and will, I think, have little difficulty in understanding them, though he may have forgotten most of his mathematics. If our reasoning has been correct and complete enough, the main principles involved may be stated as follows:—

(1). Whatever the original number of malaria cases in the locality may have been, the ultimate endemic malaria ratio will tend to settle down to a fixed figure, dependent on the number of Anophelines and the other factors — that is, if these factors remain constant all the time.[1]

(2). If the number of Anophelines is sufficiently high, the ultimate malaria ratio (m') will become fixed at some figure between 0 and 1 (that is, between 0% and 100%). If the number of Anophelines is sufficiently low (say below 40 *per* person), the ultimate malaria rate will tend to zero—that is, the disease will tend to die out. (In this calculation a *negative* malaria ratio, that is, one which is less than nothing, must be interpreted as meaning zero).

Consider, for example, the case of a village with 1,000 inhabitants and 60 different Anophelines for each person during one month. If, to begin with, every person starts with being infected, then the malaria ratio will fall month by month until it reaches the value $m = 1 - 40/60$; that is, until 333 persons are left infected. And this occurs because there are not enough Anophelines to maintain the original high rate of 100% infection. But now suppose that with the same number of people and of Anophelines, the epidemic began with only one infected person from outside: the number of Anophelines

[1] This is a well-known peculiarity of repeated functions. See, for instance, my paper "A Method of solving Algebraic Equations," *Nature*, 29th October 1908, p. 663.

will now be too large to allow this original low rate of 0·1% infection to continue, and the rate will consequently rise until the value $m = 1 - 40/60$, giving 333 infected persons, is reached. Thus, whether every one, or only one person, is infected to start with, the ultimate result will be the same—the number of cases will be 333, and will continue at that figure so long as all the factors remain the same. That is, an exact balance between the number of cases and the number of Anophelines will be arrived at. Many months may, however, elapse before this happens.

Suppose that all the factors b, s, i, a, r do *not* remain the same. Then if any of them is temporarily altered a corresponding change will be made in the rate of increase or decrease; but after the temporary disturbance has ceased, things will again tend to return to the normal state—just as water always seeks to find its own level. Thus, if the recovery rate r is increased by a temporary administration of quinine, or the number of mosquitos a is temporarily diminished by winter or by active "petrolage," a change for the better will be made, but it will cease soon after these alterations cease. If, however, the alteration is permanent, then the value of m will, of course, be permanently affected by it.

We have often assumed certain *conjectural* values for some of the factors; does this fact invalidate the reasoning? We will examine these values more closely in the next section, but the exact figures do not affect the general law. We have supposed that, in equation (7), $r/b^2si = 40$; from which we argued that the ultimate malaria ratio will vanish if the number of different Anophelines to each person is less than about 40 *per* month. The actual number may be more or less, but the fact that a limit must be reached remains. As I have said, the whole calculation is useful, not so much for its numerical results, but because it gives precision to our ideas.

A more serious objection is that, in framing the fundamental equation (4), we have disregarded certain factors which would modify it. For example, the population will be subject

to constant changes owing to immigration, emigration, births and deaths—all of which operate both on the healthy and on the infected. Thus the birth-rate will give a continuous supply of healthy individuals, while the death-rate will remove many of the infected ones. Also many of the recovered cases, and many of the mosquitos, may be at least partially immune; gametids may appear in the blood more during some seasons than during others, and infected mosquitos may bite many more than one person each; but the introduction of all these factors would give a much more complicated equation than we have need for here. It will still be of the form

$$m_1 p = \textit{original cases} + \textit{new infections} - \textit{recoveries (or deaths)},$$

and from this we shall still be able to argue that a limit must be reached when the new infections exactly balance the recoveries. We may therefore conclude,

(1) That the amount of malaria in a locality *tends* towards a fixed limit determined by the number of malaria-bearing mosquitos and by other factors.

(2) That if the number of malaria-bearing Anophelines is below a certain figure, that limit will be zero.

It is often thought and said that malaria should exist wherever susceptible Anophelines exist, and that anti-mosquito measures will therefore be useless so long as any of these insects remain. But more careful reasoning will convince us that malaria cannot persist in a community unless the Anophelines are so numerous that the number of new infections compensates for the number of recoveries.[1]

29. Laws which Regulate the Number of Anophelines in a Locality.—We have seen, then, that the amount of malaria in a locality depends (among other factors) upon the number of suitable Anophelines, and upon the proportion of them which succeed in biting human beings, in living long enough to mature the parasites, and in biting human beings again.

[1] See section 66.

We must now study these points in greater detail. Unfortunately, although much entomological work has recently been done on mosquitos, such important subjects as these have received little attention.

(1). *The output of mosquitos from marshes.*—I know of no adequate studies on this point. In 1908, in Mauritius, I stretched a mosquito-net over 9 square yards (7·5249 square metres), and counted every day the mosquitos hatched within it. The selected spot (Clairfond Marsh) was covered with rank grass, the roots of which were submerged by an inch or two (2·5-5 cm.) of water. It was sheltered by trees, and was an ideal spot for the breeding of *Myzorhynchus mauritianus* Daruty and D'Emmerez, 1900 (an Anopheline which does not carry malaria). The observations were continued for sixteen days in January (the warm rainy season), and only this kind of mosquito was found. Altogether thirty males and thirty-one females were obtained,[1] giving an average of 0·423 *per* square yard *per* diem, or 5,062 *per* 10,000 square metres. Numbers continued to hatch out on the sixteenth day. During the daytime the adults took refuge in the grass, from which it was necessary to expel them. Clairfond Marsh is about 1,400 feet (427 metres) above sea-level (section 30 (21)).

This output (about 5,000 mosquitos *per* diem for 100 yards square of marsh) seems to have been rather large, as when the net was placed in another position the yield was much smaller. Taking twenty days as the average life of an Anopheline (conjecture), 100,000 of them should thus be in existence on any one day in the neighbourhood of such a marsh of about 100 yards or metres square.

Of course the output is sure to vary from point to point of a marsh, according to the breeding capacity of different depths of water, etc.; and it will also vary from

[1] Nuttall and Shipley found equal numbers of males and females with *A. maculipennis* also [January 1902].

day to day according to the amount of rain, wind and sunshine.

Correct and sufficient observations on these points are much needed. They could be made by the use of small square tents of muslin netting, supported by a central pole, and pegged out with weighted margins over a unit of area (say 1 square yard). We might thus ascertain not only the mosquito output but the length of the aquatic life of mosquitos under natural conditions, and other facts. Of course a number of such tents must be used to avoid much error of random sampling.

(2). *The average life of the winged insects.*—It used to be thought that mosquitos feed on one occasion, lay their eggs a few days later, and then die. But in 1898, observing that this short period did not suffice to allow the malaria parasites to develop fully in *Culex fatigans*, I ascertained that these insects could be kept alive in captivity for a month, and possibly much more, by repeated feedings [21st May 1898]; and I obtained the same results with certain *Stegomyiae* and *Anophelines.* Since then, similar observations have been made on many mosquitos. Thus Goeldi [1905] kept a female *Stegomyia calopus* alive for 102 days, and a male one for 72 days. The average life in captivity of 15 females was 53 days, and of 11 males was 50 days. My general experience has been that it is easy to keep various species of Anophelines alive in captivity for two or three weeks, but not so easy to keep them longer. Nuttall and Shipley [1902] kept *A. maculipennis* for 56 days, and found that the females tend to live longer as winter approaches, possibly in connection with hibernation (Woldert). It has long been known that many mosquitos hibernate and aestivate. Females appear to live in captivity longer than males. Better experiments with large mosquito-houses, fixed in the open air under natural conditions, should be made.

But such observations do not settle the important question,

NET FOR MEASURING THE OUTPUT OF MOSQUITOS FROM A MARSH—CLAIRFOND, MAURITIUS.

BANKS OF A STREAM ROUGH-TRAINED FOR RS.0'37 A RUNNING FOOT FOR BOTH BANKS (MAURITIUS).

[*To face page* 166.

what is the *average* life of mosquitos in nature? In captivity they doubtless suffer from confinement, but, on the other hand, are preserved from their natural enemies and from heat, wind and weather, which probably destroy immense numbers of them. We should like to know what percentages survive for one, two, three . . . weeks. Numerous experiments on the point could be performed, but have been neglected. In the meantime, I have always taught the following hypothesis. The *average* natural life of an animal is likely to exceed that of any parasitic organisms which it may contain. The latter have been instructed, so to speak, by the evolution of centuries, as to the length of time they should spend over their development, so as to have the best chance of being propagated to other hosts. Thus *Filaria bancrofti* requires about three weeks to develop in *Culex*. Now, if the average life of *Culex* were less than three weeks, the filariae would have a much smaller chance of propagation. Doubtless, therefore, the average life of *Culex* exceeds three weeks or a month. Similarly, plasmodia require about ten days to develop in Anophelines, and I therefore suppose that the average life of these insects reaches about three weeks or more. But this reasoning suggests only the lower limit of average life. Evidently a longer average life would improve the chances of propagation of the parasites. The mosquito death-rate must, of course, vary largely in consequence of many factors, such as season, enemies, local conditions of shelter, etc. On the whole, I think that we shall not be far wrong if we accept our previous estimate that only about one-third of the carrying Anophelines live for ten days—long enough to allow the plasmodia to mature in them.

(3). *The proportion of mosquitos which succeed in biting human beings.*[1]—With most species only the females, that is, let us say, half the number, suck blood at all. When a number of females are liberated all night within a mosquito-net occupied by a man or by birds, only a variable proportion are found next morning to be fed. Numerous observations have been made,

[1] See also section 66.

by myself, among others, which show that the insects are not very hungry for about twenty-four hours after hatching out from the pupa, or immediately after laying their eggs. With species which attack animals (birds, cattle, dogs, etc.), the chances are that the proportion of insects which succeed in biting men varies with the proportion which exists between the number of men and of these animals in the locality. Men must be more difficult to reach than defenceless animals (or children). The richer classes often use mosquito-nets; and poor natives, especially Indians, generally cover themselves from head to foot with a sheet during sleep, besides filling their huts with smoke at night time, and keeping their doors and windows shut (the common habit, even in hot climates). Access to people in upper storeys, with small open windows, must often be difficult to these feeble insects, especially while any breeze is blowing.

I have suggested that possibly a quarter of the Anophelines may succeed in biting human beings once, a third may live for ten days more, and a quarter may succeed in biting again—that is, that only about 1/24 of the females can ever have any chance of carrying malaria. But this applies only to the proportion of mosquitos to each person in the place, and supposes, moreover, that the human beings are evenly distributed in the locality, and are fairly accessible. In thinly-inhabited places the ratio will probably be much smaller, and in crowded ones, larger. I think that many insects which have failed in procuring a meal during the night may die of starvation (in my experiments on birds a number of dead mosquitos were generally found in the nets every morning). Numbers of gorged mosquitos are probably devoured by bats, birds and spiders, or are killed by sufferers. On the whole, then, an average ratio of 1/24 is perhaps too high.

Many observers have studied the proportion of naturally-fed Anophelines which contain plasmodia. In 1899 I found the parasites in 27 out of 109 *P. costalis* caught in a military

hospital in Freetown, Sierra Leone, in which one quarter of the men examined contained them [1900]. Stephens and Christophers found them in 6/70 of Anophelines in the same town [5th April (?) 1900]; in 5-10% in Africa "as a rule"; and in some villages up to 50% [1903]. Ziemann, A. Plehn, the Sergents, and many others have found similar rates. Unfortunately, such observations do not help us much, because the mosquitos caught in houses may possibly be only or mostly those which have already obtained human food, and are waiting for more.

In the huts of poor natives, and in badly-managed barracks and hospitals where many unprotected people sleep in the same room, a single mosquito may often be able to bite several persons during the one night. In such houses the chances of infection must be enormously increased, and the practice of congregate sleeping must be one of the principal causes of the diffusion of malaria.

(4). *The number of Anophelines in unit of area.*—Nothing approaching accurate scientific work has been done on this subject. There are many references in literature to "few mosquitos," to "many mosquitos," to "swarms of mosquitos," to "des centaines de milliers de millions," and so on. Theobald [1901, I. 72] describes having twice seen clouds of male and female *C. cantans* in the English Fens, darkening the air, and producing a sound which could at times be heard a quarter of a mile distant. He says that W. W. Smith records that a train in New Zealand "passed through a wall of mosquitos three-quarters of a mile in length, twenty feet high, and eighteen inches thick"; and he mentions dense masses of gnats "like columns of smoke." If there were ten mosquitos to the cubic foot in this "wall," there would have been only 1,188,000 insects in the whole collection—not a large number considering the possible output (1). Such phenomena merely suggest that occasional "swarming" occurs with mosquitos as with other animals (including man).

Anophelines in houses have been frequently caught and counted, especially by Stephens and Christophers and recent observers in India. The numbers may vary from zero to many hundreds in a single room, especially in thatched huts. They may also vary from house to house, and according to distance from breeding waters. It is impossible to quote any correct averages. I have thought, as a general conjecture, that one Anopheline for each human occupant might be adopted as a kind of standard for comparison ; but, of course, near marshes the numbers often rise to 50,100, or more in each room, or even to each person.

We cannot estimate the number of any species in unit of area by the number caught in houses in that area, unless we know the proportions of that species found inside and outside houses respectively, at the hour of the day when the search is made. As we can never know this proportion exactly, the number of insects caught inside houses is no exact guide to the total number existing within the area of observation.

Different species appear to differ largely as to the amount of time they spend in human habitations. I define *domestic* mosquitos as those which pass a large part of their lives in houses, such as *C. fatigans*, *S. càlopus*, *M. rossii* ; though even with these species we do not know exactly how many hours they spend indoors and out-of-doors respectively. I define *sub-domestic* mosquitos as those which enter houses only for the purpose of feeding, and *wild* mosquitos as those which never enter houses at all.[1] Some species, such as *M. mauritianus*, are found in verandas of houses, but not commonly in rooms.

The average output, average length of life, and average number per unit of area of any species of mosquitos are correlated quantities which can be ascertained only by the most careful measurements, such as have never yet been attempted (so far as I can ascertain).

[1] Arribálzaga and Ficalbi give similar classifications. The latter's is based on the habits of the larvae.

In the previous section I computed roughly that malaria is not likely to persist in a locality where the pathophoric Anophelines number less than about forty different individuals to each person during a month. It would be very difficult to ascertain anywhere how many different mosquitos there are to each person, but the attempt should be made. Mere personal impressions on the point are apt to be very wrong. The victim, surrounded by many assailants, tends to magnify their number from one to ten, and from ten to a hundred. The use of a white muslin hand net, with which his enemies can easily be caught, will disclose the truth. I am inclined to adopt the following standard. The mosquitos in a room or veranda are *numerous* if a single person is attacked by more than five at a time, and *very numerous* if he is attacked by more than ten. There are few houses in the tropics where one is not solicited day and night by two or three mosquitos.

If we count the number of Anophelines found in a set of houses every day for a number of days, we could obtain the average number to be found indoors for each person living in the locality. But we cannot say that all these are *different* mosquitos. We may, however, attempt a very rough computation as follows:—Suppose that on the average there are 10 Anophelines to each person every day. Then, if all the insects are changed every day—that is, if each insect lives only 24 hours on the average—there should be about $10 \times 30 = 300$ different insects to each person during a month of 30 days. If, however, the insects live for 15 days on the average—that is, are changed twice a month—there should be about $10 \times 30/15 = 20$ different insects to each person during the month. If the insects live 30 days, there should be only 10 different ones to each person. Thus if n is the average number of mosquitos found *per diem per* person, a is the number of different mosquitos *per* person during a month of 30 days, and l is the average life of the mosquitos in days

we should then have roughly $a = n \times 30/l$. If $n = 27$ and $l = 20$, then $a = 40$, the number roughly computed as the malaria-maintaining limit.[1]

To sum up—if we think over these points carefully (and this has not always been done), we shall be convinced of the great difficulty of forming any accurate notion of the mosquito-density of any species anywhere. The enquiry would demand a laborious study of the output, which must vary from week to week; of the average life, which must also vary; and of the average numbers found in houses and in mosquito traps (section 12). It would require the services of a number of trained "moustiquiers," and the error would amount to say 10% or more, even with the most careful observations.

(5). *Variation of mosquito-density from place to place.*—It would be very interesting to determine the rate at which the mosquito-density falls at different distances from a single breeding-place. Stephens and Christophers [5th April (?) 1900] showed that in Freetown, Sierra Leone, the insects were abundant in houses near certain breeding-waters, but diminished markedly at greater distances. An exact enquiry would be very difficult. We could estimate the numbers caught in houses and traps at various distances from a single pool in an otherwise sterile area. Here one source of error, the relative domesticity of the species investigated, would cancel out from the ratios; and *ceteris paribus* the proportions caught should indicate the proportions present. But other sources of error, facilities for obtaining food and shelter in different houses, might disturb the results. An empty farm close to a marsh might, for instance, attract many fewer insects than a small, crowded village half a mile away. Nevertheless, we know that *as a broad general rule*, mosquitos tend to diminish with distance from their breeding-place; but the exact curve of diminution has not been ascertained.

(6). *Variation of mosquito-density from time to time.*—Here, too, we could count the number of mosquitos in houses and

[1] See, however, end of (13), section 66.

traps for a series of days, and the factor of relative domesticity would be eliminated from the ratios. The total variation obviously depends upon two factors, that of output and that of longevity. If both the birth-rate and the death-rate are increased or diminished, the total density might remain the same, a fact which seems sometimes to have been forgotten.

(7). *Variation of mosquito-density due to food, etc.*—Abundance of food certainly has a great effect on domestic Culicines, which tend to swarm in crowded and poor habitations. Probably it has a similar effect on sub-domestic Anophelines, though these may not be so much in evidence. I mean that abundance of food probably tends to increase the output, although the total breeding surface remains the same. This would be due to the fact that the females find food more easily and consequently lay more eggs. Thus malaria may perhaps increase in a locality, not because of the increase of breeding-places or rainfall, or because of the introduction of more imported cases, but simply because an increase of the human population has provided more food for the Anophelines (section 30 (9), (12), and (21)).

(8). *Relation of mosquito-output to extent of breeding-surface.*—This is a point of great importance as regards prevention. If we reduce the extent of breeding-surface to a given proportion, what will be the exact effect on the number of mosquitos?

(*a*) The number of larvae in a collection of water will depend (1) on the number of eggs laid in it, and (2) on the suitability of the water — temperature, shelter, absence of enemies, food.

(*b*) Probably that number cannot exceed a certain limit; that is to say, there must be a maximum possible output per unit of breeding-surface at any season.

(*c*) Probably also the actual output is often less than the maximum possible output, the deficiency being due to absence of enough food for the females,

destruction of the adults, inaccessibility of the water, etc.

(*d*) The maximum possible output per unit of breeding-surface is likely to vary with season, and to be much greater during the warm season when the food of the larvae is probably more abundant, and their development quicker.

From these data we infer as follows:—

(*e*) Suppose that the breeding-surface of a locality is yielding the maximum output, and is then suddenly reduced in extent, say to half or a quarter the previous area. Then a proportional fall must occur in the total output of mosquitos in the locality.

(*f*) Next, suppose that the breeding-surface was yielding only a fraction of the maximum possible output when the extent of it was reduced. Then the fall in the total output of mosquitos may not be so great. The females, which formerly laid their eggs in the part of the area which has been drained, may now resort to the pools which are still allowed to remain, and may increase the output of these by stocking them with an additional number of eggs. Thus, though the total breeding-area is reduced, the part of it which remains may have a larger output; so that the total output in the locality may remain the same as before.

(*g*) But this compensation has its limits. If the reduction of breeding-area has been great, the pools that remain may often be inaccessible to the females, or may become overstocked with eggs and larvae, for which they cannot provide enough food. At best, the total output cannot exceed the maximum possible output of the waters which remain.

(*h*) If the drainage operations have been commenced early in the season, before the breeding-season is fully

developed, and if they have not been complete, then, as the breeding-season advances, the waters which remain will still continue to have an increasing output; so that the total output of mosquitos in the locality may continue to increase in spite of the partial drainage operations. I say that, if any breeding-waters at all are left, the output will increase from month to month as the breeding-season advances to its maximum; but this does not mean that the total output after the drainage operations is not less than the total output in the previous season before the operations. The thoughtless observer, seeing an increase of mosquitos due to the seasonal increase of output in the waters left undrained, may jump to the conclusion that the drainage has had no effect. But a comparison of the total output before and after the drainage would probably correct this error. The two issues have frequently been confounded — as, I think, at Mian Mir. Of course, if any breeding-waters at all remain, a certain number of mosquitos will continue to be poured out, especially at the height of the breeding-season; but it is scarcely likely, *ceteris paribus*, that a small breeding-surface can have as great an output as a large one, and it certainly cannot have more.

We conclude then as follows:—

(1). That *ceteris paribus* the output must tend to vary with extent of breeding-surface.

(2). But that the two curves will not always exactly coincide.

For example, if the breeding-surface is reduced to a half, then the output of mosquitos will also be probably reduced very considerably; but it may not be reduced exactly in the same proportion. If the breeding-waters are entirely removed, then, of course, the output in the locality must entirely cease.

(9). *Flock-migrations of mosquitos.*—We have hitherto considered the mosquito population of localities as if it consisted solely of insects born in the locality. But, obviously, many of the insects found in any area must have entered from without; and we must now examine the subject of mosquito-migration.

By *flock-migration* I mean the simultaneous movement of large numbers of animals of the same species in the same direction — such as we are familiar with in the case of wild cattle, swallows, locusts, etc. Does anything of the kind occur with mosquitos? Howard [1901] quotes a letter in which flock-migrations of immense numbers of mosquitos are reported to have been twice witnessed by the same observer in America. It was supposed that the insects had originated in a large marsh 35 miles distant; and they were numerous enough to cloud the sky, and bend down the grass with their weight. Nuttall and Shipley conjecture [January 1902] that the phenomenon may be due to "overstocking of a given locality by a species." If it has occurred once, it ought to occur frequently enough to be recorded more often. There is always the danger that a large local hatch-out may be responsible for the occurrence.

(10). *Visitation of ships.*—Mosquitos frequently visit ships half a mile or more from the shore. We must not infer that they have purposely travelled so far in search of food. Winged animals which have once started on a flight across water seldom have the sense to return. At Highcliffe, England, I once watched numbers of butterflies (*P. brassicæ*) flying out to sea from the shore on a still morning—none were coming back. Birds and insects, lost in this manner and wearied with flight, naturally board passing ships for rest. Sometimes, however, a ship anchored close to shore may be attacked by mosquitos which have perhaps scented their prey from a distance. On the other hand, I have often been on board such ships without noticing many mosquitos. Once I spent a night in a small open boat, rowed down a river in Burma, but observed at the

time that not a single mosquito attacked us, though the night was still and warm.

(11). *Transportation by ships and vehicles.*—I have known *Culex* and *Stegomyia* to breed in water-jugs and flower vases on board ship, and they frequently breed in bilge-water and wooden water barrels. But as soon as the ship starts on her voyage most of the insects seem to be blown out of the cabins. Nuttall [1899] says that Roe once observed a dozen foreign species of mosquitos on board a ship lying at quarantine in New York. This was probably a sailing ship, as the vibration of steamers seems to prevent the insects coming to rest. Balfour notices that Anophelines are brought into Khartoum in boats.

Every one who has lived in the tropics has observed that mosquitos occur in carriages and railway trains. During the heat of the day the insects seem unwilling to leave the vehicle, though it is in motion, and may thus be transported considerable distances. But vehicles which introduce mosquitos may also remove them, and it is absurd to suppose that the small numbers carried in this way can often influence the malaria rate.

(12). *Transportation by rivers and winds.*—Nuttall, Cobbett and Strangeways-Pigg [1901] suggested that rivers may transport eggs, larvae and pupae, but I think that most of the latter would be devoured before they have travelled very far.

Many people imagine that mosquitos are carried by winds. Householders who breed them in their own premises like to ascribe their presence to marshes some miles away. The authors just quoted state [1901, p. 8] that Fernald described how mosquitos at Cold Spring Harbour, Long Island, New York, were blown there from a distance of 15 miles by the south wind ; but Nuttall and Shipley [1902, p. 61] quote Weeks as correcting this by showing the existence of many breeding-places at Cold Spring Harbour itself. When I arrived at Freetown, Sierra Leone, in 1899, every one thought that the local mosquitos came from marshes a mile or more distant : they were really being bred in every house in the town. I

could mention many more experiences of the same kind. Statements regarding wind-transportation must be received with great reserve, because here, also, a local hatch-out may be responsible for the facts stated.

In Mauritius in 1908 I placed pickets of trained "moustiquiers" at different distances from Clairfond Marsh with instructions to catch all the Anophelines which attacked them. The men sat out late at night and brought us their captures in the morning. Their conclusions were that the insects scarcely move about at all when there is much wind. On still, warm nights, however, especially after rain, the pickets caught both *P. costalis* and *M. mauritianus* half a mile from the marsh. These facts are opposed to those who believe in frequent wind-transportation, but accord with the views of Howard and others, and myself.

Occasionally, of course, a few insects may be swept away by wind, but in most cases wind merely drives them into shelter. In fact, whenever there was any wind our "moustiquiers" caught adults only in sheltered spots behind houses, hedges or woods. I fancy that gnats, like other animals, dislike being swept away from their own haunts into unknown regions.

I have always used the following good argument against the notion that wind transports mosquitos to any great extent. If this were so, sea-shores constantly swept by sea-breezes ought to be nearly free of them; but I can certainly bear witness to the fact that this was not the case in Madras and Port Said, when I was in those towns in 1881-1884 and 1902. A strong sea-breeze blowing through a house with all the windows and doors open will not, in my experience, drive out the Culicines, nor even reduce their numbers.

(13). *Velocity and length of flights.*—I am not aware of any experiments on these points, but I have frequently observed *Stegomyiae* flying against breezes which I judged to be blowing at over five miles an hour. They can easily follow a man walking, and perhaps a trotting horse. I suppose that they

fly at the rate of about eight miles an hour, roughly computed.

At this rate an insect might easily cover ten or twenty miles a day, and *if it were always to fly in the same direction from its breeding-place*, might traverse a hundred miles or more during its life.

Many absurd statements, such that mosquitos can fly only half a mile or so from their breeding-place, continue to be made. It is not a question of what is their power of flight, or how far they can fly, but of how far they actually *do* fly *on the average.*

My proposals to reduce malaria by dealing with the breeding waters were long ridiculed because it was thought that as soon as the local output was checked mosquitos would rush in from outside to fill up the deficiency. Several eminent biologists held this opinion, and a ridiculous experiment was undertaken, apparently in the hope of proving it. In 1903-1904, however, I attempted a carefully reasoned consideration of the whole subject—so vital to the interests of malaria prevention; and showed in a lecture [May 1905] that the average wanderings of the insects, and indeed of all animals, must be limited by laws of chance. I will now explain that reasoning as simply as possible.

(14). *The random scatter of animals from a given point.*—Suppose that an animal is liberated at a given point—for instance, a mosquito from a box—and suppose that it can find its food equally well anywhere in the surrounding country, and is not drawn towards any particular spot, or driven anywhere by wind or other things; what are its movements likely to be? We can imagine that it may *just possibly* continue always to move in the same direction, so that when it dies it will be found at the greatest possible distance from the point where it was liberated. Or it may move for half its life in one direction, and for the other half of its life back again—so that it will die actually at the point where it started. But both these courses

will scarcely ever be adopted. In the vast majority of cases, the animal will move for a short distance, first in one direction, then at an angle in another direction, then again at another angle, and so on until it dies. Its movements will resemble those of a grain of dust placed on a level plate in a jolting railway carriage; or the random walk of an intoxicated person in a mist; or that of a cow grazing on a uniformly succulent meadow. What are the chances that when the animal dies or ceases to move it will be found at a given distance from the starting-point?

Obviously, it will most probably be found somewhere near the starting-point. There is no reason why it should move more in one direction than in another. The chances are equal that, at any change which it makes in the direction of movement, it will next move north, south, east or west. Hence its various movements will always tend in the long run to annul each other—so that it will tend to finish near where it began. But there is no *certainty* that its various movements will annul each other *exactly*; hence, most probably, it will not be found exactly at the starting-point, but only somewhere near it.

Or we may put the problem thus. Suppose that a million Anophelines are liberated from a single breeding-pool in the midst of a country where they can obtain food equally well at any point, and suppose that we know their average rate of movement and length of life: how many of them will be found at a given moment at a given distance from the pool? Most of them will be found, not exactly at the breeding-pool, but close to it. A few will be found further away, and a very few at the extreme limit of possible flight.

What precisely will be the ratio of insects at a given distance from the pool? In my lecture just mentioned I attempted a partial mathematical treatment of the problem, but the matter was beyond my mathematical powers, and I therefore referred it to Professor Karl Pearson, who, with J. Blakeman, obtained

a correct and complete solution, based on the laws of chance [1906].[1]

It is not possible to give their complicated mathematical analysis here, but I will presently mention some of their results. Besides its application to the prevention of mosquito-borne disease, the mathematical theory of migration has manifest and important applications in general biology, as, for instance, to the theory of evolution and the study of local variation of race.

The general results obtained from the calculations may be put as follows:—

(*a*). Unless mosquitos are drawn or driven in any particular direction or directions, their number will tend to be greatest somewhere near the breeding-pool, and to diminish progressively at greater distances from it.

(*b*). *Per contra*, if the mosquitos are very numerous, then, *ceteris paribus*, the breeding-pool is likely to be near at hand.

These laws are confirmed by the general observation of many workers and of the public. For example, Stephens and Christophers [25th April 1902] conclude that the "flight" of *M. culicifacies*, *N. fuliginosus* and *N. stephensi* in Nagpur, India, is "frequently a quarter of a mile, but does not extend to half a mile."

(15). *Exceptions.*—These will be due (*a*) to flock-migration, if it occurs; (*b*) to carriage by vehicles, boats and wind; and (*c*), more generally, to food-pursuit.

The law of random scatter applies only if the country affords equal facilities for feeding in all directions, but if this is not the case, the insects must, of course, go where they can find their food. Thus if there is only one breeding-place and one feeding-place somewhat far apart, the females must always traverse that distance between feeding and laying their eggs; and if the distance is great, probably few of the females will succeed. If there is only one feeding-place in the midst of many breeding-places, such as a village surrounded by marshes,

[1] This paper is not mentioned in the medical text-books.

then quite possibly all the females which can reach the feeding-place will focus themselves upon it.

Here a law which has not been investigated, though it is most important as regards prevention, comes into play. What is the maximum distance at which a female mosquito can scent her human prey? If this is one mile, then, in the case of the single village, drainage for the purpose of freeing it from mosquitos would have to be carried out for a radius of one mile round; if only half a mile, then for a radius of half a mile; and so on.

(16). *Diminution of mosquitos round a central sterile patch.*—Suppose the case of a plain with numerous breeding-places scattered uniformly over it, and suppose that there is a central sterile patch in it—that is, a patch where, owing to natural or artificial causes, the insects cannot breed: how will this patch affect the mosquito density round it?

The answer is obviously that the mosquitos round the patch will be diminished by exactly the same numbers as the patch would have produced if it had been allowed to continue yielding an output of mosquitos. That is, by the law of random scatter, the diminution will be greatest over the patch, progressively less as we recede from it, and none at all at a sufficiently long distance from it.

If the patch is very small, the diminution will be very small, even at its centre, and will be hardly appreciable at a short distance from it. Here, of course, numbers of mosquitos will be able to fly across the patch.

If, however, the patch is a large one, few of the mosquitos migrating inwards from its margin will be found, according to the law of random scatter, at its centre. Suppose, for instance, that by this law only 1/100 mosquitos are found 1,000 metres from the centre of a breeding-pool; then conversely, in a sterile patch of 1,000 metres' radius, only about 1/100 of the mosquitos breeding at the patch's margin will be found at its centre.

The diminution in mosquito-density due to a sterile patch

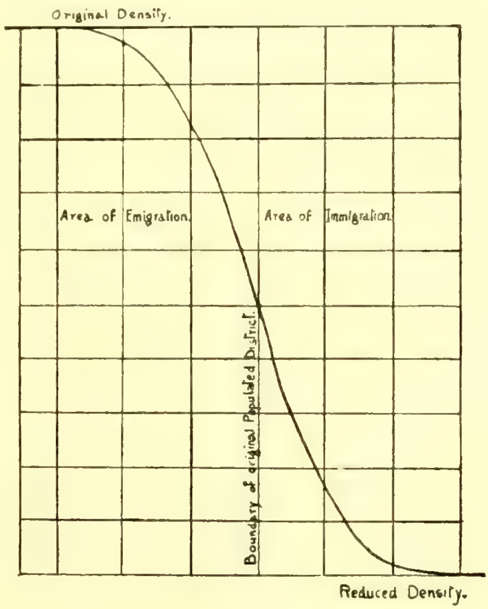
will be felt not only over the patch itself, but to a considerable distance beyond it. That is, the mosquitos will be diminished just so far and by just so many as the patch would have put out by random scatter if it had been an isolated breeding patch, and the surrounding country had been sterile.

That is, the diminution of density due to an isolated sterile patch and the increase of density due to an isolated breeding patch must always be, *ceteris paribus*, equal.

From this it follows, in the case of a very large isolated sterile patch, that the mosquito-density exactly at the margin of the patch should be exactly one-half the normal mosquito-density. This law was stated in my paper, and has been confirmed by Pearson and Blakeman.

I gave also a diagram showing the fall of the mosquito-density on either side of the straight boundary of a large sterile patch, and now reproduce the more exact curve shown by Pearson and Blakeman for certain constants roughly estimated by me.

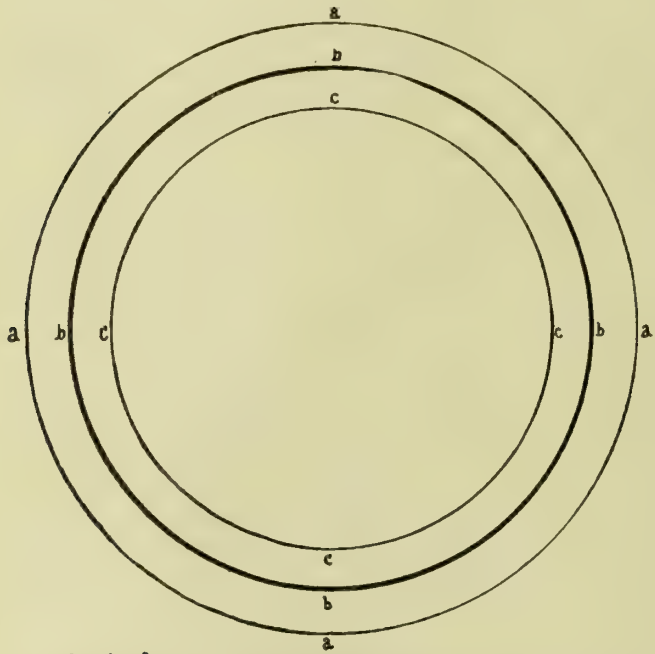
FIG. 2.



To understand this, we suppose that the mosquitos (or other animals) are breeding as usual on the left—that is, in the Area of Emigration, and that many of them are wandering across the boundary into the sterile area (Area of Immigration). The curve beginning on the left shows the fall of the mosquito-density. Slow at first, it increases rapidly as we approach the boundary from outside. Exactly at the boundary it is exactly one-half. Inside the boundary it continues to fall, rapidly at first, and then more slowly as we approach the centre of the sterile area. At the centre, if the patch is sufficiently large, it should be nearly or practically zero.

The following diagram from my paper shows the same thing *in plan.*

FIG. 3.



Effect of drainage of a circular area, b, boundary of drained area. Mosquito-density begins to diminish at the circle, a; becomes one-half at the boundary, b, and is small, inappreciable, or zero at and within the circle, c.

We should also understand the following simple propositions. If a straight line is drawn across a country in which mosquitos (or other animals) are wandering equally and at random, then as many will wander from left to right of the line as from right to left. Moreover, the longer the line the more animals will wander across it, *ceteris paribus*, in unit of time. This should hold also for circles. Thus, if we draw two circles, one 100 metres in radius and the other of 1,000 metres, ten times more animals should wander across the circumference of the latter than across that of the former. Thus the number of immigrants into the large circle should be ten times the number into the small one. But the mosquito or animal *density* is obtained by dividing the total number of animals by the size of the area; and the area of the large circle is $10 \times 10 = 100$ times that of the small one. Thus, though ten times more mosquitos actually wander into the large circle, yet the average mosquito density *due to immigration alone* in the large circle will be only one-tenth that in the small circle. Now if both these circles contain sterile areas, the mosquitos within them can consist only of immigrants. Hence the *total* mosquito density in the large sterile patch will be only one-tenth that in the small one. In other words, *the larger the sterile patch the smaller will be the mosquito density due to immigration into it from outside.* That is, the larger the area of drainage the more effective it will be.

It has often been thought, absurdly enough, that the mosquitos in an area may not always be reduced by rendering it sterile. This can never happen *ceteris paribus*, because the number of immigrants can never equal the number of immigrants *plus* the number of natives. But if the area is very small it may breed so few natives that the reduction after it is sterilised will not be noticeable. But by suitably increasing the radius of the sterile patch we can reduce its average immigration density to any small figure we please.

Of course, the immigration density is not uniform, but,

as already stated, begins at the boundary, at a fraction of the normal density, and from that fraction diminishes rapidly towards the centre, where, *if the sterile patch is* large enough, it may become practically zero.

Pearson and Blakeman have made some exact estimates based on rough constants supplied by me. For example, in a circular sterile patch one mile (1·6 kilometres) in diameter the mosquito density should be reduced at the boundary to about 75% of the normal density; to 18% a quarter of a mile from the boundary; and to 30% at the centre. If the sterile patch is two miles long and one mile broad, the central density should be 1%. In a sterile square mile, the density should be 50% at the boundary, 11% half-way to the centre, and 2% at the centre. In section 28 I attempted to calculate the probable fall in the malaria rate which may result from a decrease in the number of mosquitos.

30. Explanation of Various Phenomena.—In the previous sections I have endeavoured to give the general theorem of the spread and prevalence of malaria. We have now to apply this theorem to the explanation of numerous phenomena which have been observed in connection with the epidemiology of the disease.

From section 28 (equations 4 and 7) we have the two following formulae:—

$$\textit{Variation Formula, } m_2 = m_1 + b^2 sia\,(1 - m_1)\,m_1 - rm_1$$
$$= m_1 + 0{\cdot}005\,a\,(1 - m_1)\,m_1 - 0{\cdot}2\,m_1 \text{ (say)}$$
$$\textit{Static Formula, } \quad m = 1 - r/b^2 sia$$
$$= 1 - 200r/a \text{ (say).}$$

The *first* formula suggests the manner in which malaria *varies* in a locality. Here m is the proportion of infected persons at the beginning of the enquiry; a the proportion of different carrying Anophelines to each person (healthy or infected) during the enquiry; r the rate of recovery; b the

proportion of Anophelines which succeed in biting men; s the proportion of Anophelines which succeed in maturing the parasites; i the proportion of patients with gametids in their blood; and, finally, m_2 the proportion of infected persons at the end of the period of enquiry. Here m_1, r, b, s, i, m_2 are all fractions. I suppose conjecturally that if the enquiry lasts one month $b^2si = 0{\cdot}005$ (about), and $r = 0{\cdot}2$.

The *second* formula suggests the *final or static level* m, to which the proportion of infected persons falls so long as the proportion of Anophelines, a, and the other factors remain about constant.

The exact numerical values of b, s, i, do not materially affect the general argument. The equations omit several minor factors, but are still useful for giving precision to our ideas.

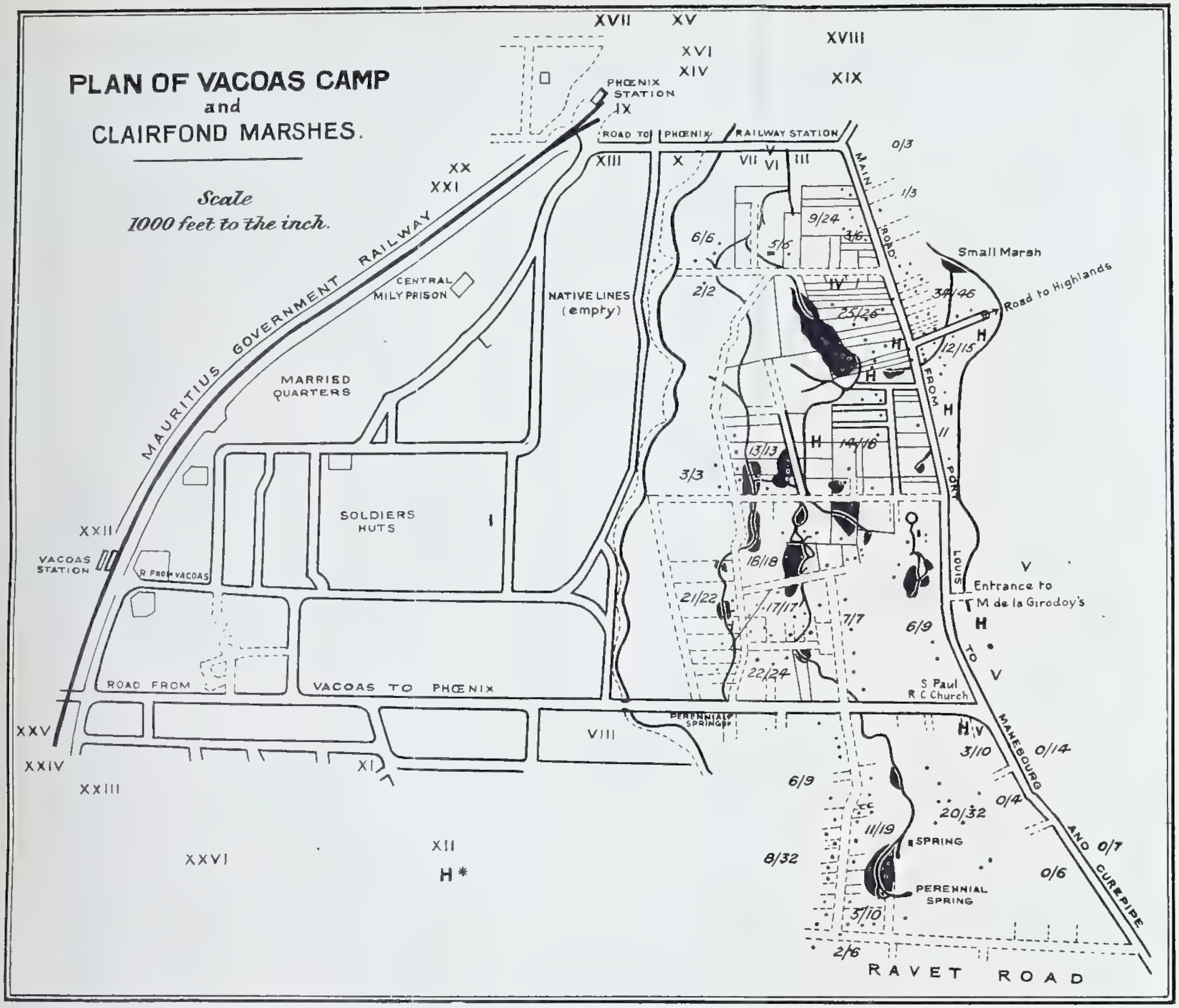
(1). *Connection with marshes.*—The fact that malaria tends to be very prevalent near marshes was observed by the ancients, has given the name *paludism* to the disease, and is perhaps the most fundamentally important fact of our subject. It is easily explained because most Anophelines breed principally in marshy waters, and because, by the law of random scatter in (14) of last section, they will generally tend to be most numerous near their breeding-pools. By the static formula, the larger a becomes, the more m approximates to unity—that is, almost the whole population becomes infected. *Per contra*, the further we recede from an isolated marsh or other isolated breeding area, the smaller the mosquito density becomes, and m falls in value. When we reach such a distance from the marsh that a becomes 40 (conjecturally) or less, the static malaria tends to disappear altogether.

Of course there are many factors which may possibly modify the exact paludo-peripheral distribution of malaria; such as the existence of abruptly-rising ground, or of open water, or of thick forest close to the marsh; or (?) a strong wind continuously prevalent in one direction. Species of Anopheline may affect the radius of distribution. Where there are many

scattered breeding-places within range of influence of each other, the malaria will tend to be more equally distributed.

The fundamental fact has been noticed, if not studied, everywhere, especially in Italy. Stephens and Christophers [April 1902], in a paper on the factors which determine endemicity, conclude from a number of scattered observations in India that "other things being equal, there is a direct relation between the extent and proximity of breeding-grounds, the number of Anopheles in the houses, and the endemic index." Unfortunately they give only ratios, and these only for entire villages at various distances from the breeding-grounds. In 1908, C. E. P. Fowler and I made an exhaustive study of spleen rates of children in houses scattered close round the Clairfond Marsh in Mauritius (section 30 (21), and map opposite). We found an extremely rapid fall in the spleen rates even at a distance of 200-300 metres from the principal pools of the marsh. In the three rows of barracks of the British soldiers, the nearest of which was 850 yards (777 metres) from the nearest pool of the marsh, and the furthest about 200 metres further, Colonel Peterkin, the Principal Medical Officer, found that the ratios of malaria cases to men were 31/206 = 150/1,000, 22/252 = 88/1,000, and 2/62 = 32/1,000, in the successive rows. Many other instances might be cited from the literature.

The definition of the word "marsh" as here employed is a very important point. Webster, for instance, defines a marsh as "a tract of soft wet land commonly covered partially or wholly with water." This definition includes the marshy borders of rivers and lakes, etc.; but we must extend it, for the purpose of our present technical meaning, to exclude the idea of *permanence*. In our sense, marshes may be quite temporary—may exist only for a few weeks during the rainy season, or season of floods, or wet cultivation. The reader must not imagine that Anophelines breed only in permanent marshes. I have often thought that water-logged country



Enlargement of spleen in children living round Clairfond Marsh. The principal pools are shown in black. The fractions give the spleen ratios: thus 9/24 denotes that 9 out of 24 children had splenic enlargement.

[To face page 188.

may often be really less favourable to them than a usually dry country which may be covered during the rainy season by extensive temporary marsh. In permanent pools, the larvae probably have many enemies — fish, beetles, tadpoles, other larvae—which cannot live so easily in the shallow, evanescent pools caused by rain at certain seasons. It is quite possible that for this reason a unit of temporary marsh has an actually larger annual Anopheline output than a unit of permanent marsh, and this consideration may help to explain the great prevalence of malaria in certain dry countries, as in the north of India.

(2). *Connection with soil.*—Obviously, an impermeable soil is likely to possess more collections of surface-water than a loose, porous one, and thus to increase a, the number of Anophelines. A sub-soil stratum of rock or clay has often been mentioned as increasing malaria.

It is possible, also, that certain soils are favourable to the breeding of the local malaria-bearing mosquitos, while others have the opposite tendency.

There is much old literature on this point—written when malaria was attributed to the soil. After considering it, I think it is scarcely worth reference, although much of the work was carefully done. There is never any proof that the variations of malaria attributed to differences of soil were not really due to other causes. I am far from saying that soil exerts no influence on the endemicity ; but I can find no decisive evidence that it does so, apart from the merely mechanical effect of permeability. The matter deserves more exact study by modern methods.

(3). *Connection with slope.*—Abrupt hillsides have little malaria as a rule, but Anophelines often breed in dry beds of torrents, as (for instance) described by F. Smith and A. Pearse in Sierra Leone [1904]. I was infected in 1897 in such a place, Kalhutti, near Ootacamund, India [February 1898], and the native servants of the house were attacked there also. Small flat valleys among

mountains are apt to be very malarious. Slope acts mechanically on the drainage, but the labour and heat of climbing predispose to chills and relapses.

Flat plains at the foot of mountains—called *terai* in India—are notoriously unhealthy. This is due to the fact that they receive all the surface-drainage of the rain on the mountains, the water flowing from which immediately stagnates on the flat levels. After heavy rain a whole mountain seems to ooze with moisture at its base, springs often appearing considerable distances away on the plain.

Whether mosquitos often find their way far up slopes is doubtful. In 1899 we observed scarcely a single Anopheline in the barracks at Tower Hill, a small open hill in the centre of Freetown, Sierra Leone, with numerous breeding-pools only about 500 metres distant. Infections among people living at a small height on slopes is probably more often due to the visits they pay to the plain, rather than the visits paid by the Anophelines of the plain to them.

(4). *Connection with vegetation.*—Several Anophelines are known to breed in plants and trees. Certainly, many Culicines do so, and also seem to like the shelter from sun and wind given by dense vegetation. Our "moustiquiers" in Mauritius had no difficulty in securing Anophelines in the densely-wooded "river reserves." But I do not know any numerical researches which have been made to prove that any malaria-bearing species abounds more in woods than on open ground (see, however, sections 57 and 63).

It has been proved statistically by Mr Walter of the Mauritius Observatory that the damp exhaled by trees increases the number of rainy days, and especially the afternoon rainfall so frequently seen in the tropics. For this reason alone, therefore, trees should favour the breeding of mosquitos.

It is generally held that a screen of trees shuts out malaria and mosquitos—this being one of King's original arguments in favour of the mosquito theory. Stephens and Christophers

also accept the hypothesis. It is a likely one, but better proof is required.

The fact that the true *Plasmodiidae* have hitherto been found only in men, monkeys, bats, squirrels and perching birds is a curious one, which suggests an arboreal connection.

(5). *Connection with rainfall* is manifestly due, in the case of *summer* rain, to the increased mosquito output. Rain also tends to bring on relapses, and therefore to increase the factor—since patients with frequent relapses tend to show more gametids than those without them (section 20 (6)). Thirdly, I think that it increases the biting factor, b, and quite possibly reduces the recovery factor, r. Hence, on all counts it must tend to increase the malaria.

Winter rain may possibly reduce r, but, if the winter is cold enough, can have little other effect. Thus in Greece most of the rain falls in the winter, when there is little new infection.

Much *spring* rain, however, has a very disastrous and well-known effect, as it fills the pools just when the weather is becoming warm enough for breeding. This has been especially noted in Greece.

Statistics support the common statement that malaria inoculation occurs most frequently at the beginning and the end of great summer rains. At the height of these rains, when they are copious, the ground is often covered with more or less running water, frequently containing myriads of small fish, and too disturbed for much breeding.

On the other hand, in countries with small total summer rainfall, the maximum breeding is more likely to occur at the height of it. There was a bad outbreak in the north of India in 1908, when most of the rainfall was concentrated in the month of August.

Innumerable statistics showing the connection between rainfall and malaria might be reproduced here. Unfortunately, while they prove the existence of the connection, they do not enable us to discriminate between inoculations and relapses.

Most probably the variation formula is affected by changes in all the factors mentioned above.

(6). *Connection with temperature*, like that with rainfall, is probably due to increase of all the factors and decrease of the recovery factor r consequent on exposure to heat or sun. Warmth is, of course, an essential to copious breeding, but great dry heat must tend to desiccate the pools.

An important question remains to be considered. In low latitudes the temperature is generally uniformly hot all the year round, so that the Anophelines should be able to breed at all seasons, especially where the rainfall is also fairly evenly distributed. But in higher latitudes the breeding can occur only during the short hot weather. Hence we should infer that the Anopheline factor must always be higher in the former, and the malaria more abundant. But statistics often show that just the opposite happens. For example, in the north of India, where the winters are sharp, the total fever rate is generally considerably higher than in the south, where there is no winter. Malaria is, or was, intense at Peshawar in the extreme north, and scarce in Calcutta, Madras, Rangoon and Colombo. But this law does not always hold, for malaria is common in Panama, Colon, Lagos, Freetown and Port Louis (Mauritius), all of which have a climate very similar to that of the four Indian coast towns. (I write from personal acquaintance).

The probable explanation is as follows. So far as we can judge, not all the Anophelines can carry malaria, and various carrying species differ in carrying power, thus modifying the carrying factor, s. It does not follow that of all the Anophelines of a country those with high carrying power breed best in a uniformly warm climate. Stephens and Christophers showed this well in their paper just referred to. Thus *M. rossii*, with a low carrying power, prevails most in Calcutta and Madras. See (11) below.

(7). *Connection with altitude.*—It is well known that malaria

tends to diminish and cease at an altitude of about 500-1500 metres above sea-level. The exact limit probably depends upon the latitude. I was infected in 1897 at 1,800 metres in the Nilgiri mountains in India. The disease abounds at Cilaos, Réunion, at 1,214 metres. The text-books quote many similar cases, but these often require verification. It is more interesting to ascertain the gradual fall in the malaria curve with altitude. Stephens and Christophers, misled, I think, by insufficient random sampling, thought that altitude under 4,000 feet (1,219 metres) "does not seem to play an important part," but our copious figures of spleen rates in Mauritius (section 22) prove that it does. That island, consisting almost everywhere of plateaus sloping gradually downward and therefore being capable of breeding at all the altitudes (from 0 to 549 metres), gives an excellent opportunity for the enquiry, as will be seen by study of the table referred to. The carrying Anopheline was probably entirely *M. costalis.*

As is well known, the temperature of the air tends to fall about 1° F. for every 300 feet of altitude (about 0·56° C. for 100 metres). The general decrease of malaria with altitude is probably due mostly to this fall in temperature, but in Mauritius, and perhaps in many other places, other factors besides temperature may retard the breeding of the local carriers at the higher levels. The following table, calculated from that in section 22, gives the spleen rates and average spleen for groups of altitudes:—

altitudes (feet)	0-300	3-600	6-900	9-1200	12-1500	15-1800
fall of temperature	0° F.	1	2	3	4	5
spleen rates	44·5	41·2	31·5	8·5	10·2	4·7
average spleen	2·98	3·08	2·03	1·23	1·56	1·22

The exceptional figures at 1,200-1,500 feet were due to the epidemic round Clairfond Marsh. Apart from these, the fall, both in spleen rates and average spleen, is not noticeable under 600 feet, and then becomes very rapid up to 1,200 feet—after which, under normal circumstances in Mauritius, the malaria

is slight, or possibly only imported. Other facts will be elicited when governments get into the way of collecting spleen rates annually, as they should do.

(8). *Comparative freedom of centre of towns.*—It is generally stated as a commonplace that malaria abounds less in the centre of towns and more in the suburbs; and, from my own *general observation*, this appears to be the case in all the larger towns known to me. But the matter requires strict enquiry by measurements of malaria made on proper principles from the centre outward.

The probable explanation is that the Anophelines, though they may breed in small numbers in wells, cisterns, gutters, waste from water-taps, etc., in towns, yet cannot generally propagate so copiously and freely there as in the more open regions in the outskirts. Probably, also, as the wealthier people generally live in the better built and paved centre of a city, the recovery factor is increased and the gametid factor decreased there by the good medical treatment usually accessible to these classes. But there is also the following factor.

(9). *Effect of density of human population.*—Suppose that in a locality the mosquito population remains the same, but that the human population varies: what will be the effect of this variation on the malaria ratio? By the static formula $m = 1 - 40/a$; but a is the number of Anophelines, not in unit of space, but *per* unit of human population. If, therefore, the latter is doubled while the total mosquito population remains constant, a will be halved; and so on. Thus the static malaria ratio tends to decrease with increase of the density of the human population. That is, other things being equal and the Anophelines being supposed to breed equally everywhere, the malaria ratio should be higher amongst a scattered rural population than in a dense urban one, because, evidently, the number of Anophelines *per* person will be less in the latter.

But I am not sure that if the human population varies, the mosquito population *will* generally remain the same. Unless

the latter find abundance of food independently of the former, their numbers are likely to diminish if the former diminishes (section 29 (7)). This will depend largely upon whether the local carrier is a domestic, sub-domestic or wild species, and on other circumstances.

If the total mosquito population varies directly with the human population, the factor a, and therefore the static malaria, should remain constant — that is, change of density of the human population will not affect the result.

If the mosquito population diminishes as the human population increases, the malaria ratio should fall greatly—as in well-drained towns. If the former increases with the latter but more rapidly, the malaria should increase.

It may happen that when the human population begins to increase the local breeding surface is already yielding its maximum output of mosquitos. In this case the increase of the human population should cause a decrease in the static malaria ratio (section 29 (8)).

In all these cases the malaria *ratio* is not, of course, the same thing as the total number of patients.

If the local carrier belongs to a species or variety which feeds almost entirely upon man, and if the human population is greatly reduced, it may perhaps follow that this species can no longer continue to thrive in the locality—so that the malaria should die out. Dr Castel showed me a large marshy area in Mauritius which, he said, was formerly thickly inhabited; but the disease became so prevalent there that the people deserted it in large numbers. Now it contains only a few scattered huts, the occupants of which show a low spleen rate (with *P. costalis*).

(10). *The "regional factor."* — It often happens that two neighbouring tracts of country, apparently similar in all respects such as climate, breeding surface, habits of people, differ largely in the malaria ratio. Stephens and Christophers [25th April 1902] attributed such variation to "undefined causes which we have termed the *regional factor*. The regional factor

may be largely due to species, but more accurate and detailed observations on the distribution of Anopheles and malaria are necessary before this can be decided." It may be due to many small things which, though not very apparent to the observer, may largely affect the mosquito factors *b*, *s*, *a*. Thus the local carrier in the larval stage may require a certain kind of food which abounds in water lying on certain soils present in one locality and not in others. Again, certain soils may favour special enemies of the larvae of the carrier—small fish, beetles, cannibal mosquito larvae, parasites; while certain classes of vegetation may favour enemies of the adults. All these are likely to be very potent causes of variation in the mosquito factors, though they may not be easy to detect without long enquiry. If we attribute the regional factor merely to "species" of carrier, we have still to "explain our explanation," since we must show why a given species abounds more in one area than in another.

Hence I define as follows. By *regional factors* I mean all those local conditions, apart from mere extent of breeding surface, which influence any of the mosquito factors *b*, *s*, *a*; that is, local conditions which influence the output *per* unit of surface, the biting power, and the maturing power of the local Anophelines.

(11). *The species factor.*—This influences *b* and *s*, the biting and maturing powers of the carriers, which probably differ largely with the species or even variety of the carrier. Where these factors are high, a lower number of Anophelines will produce a given amount of malaria.

Thus tropical Africa appears on the whole to be more malarious than India—that is, the chances of becoming infected in unit of time and season in Africa are said to be greater in Africa than in India. This may be because good carriers such as *P. costalis* and *M. funesta* abound more in the former. In India the bad carrier, *M. rossii*, seems often to crowd out the more pathophoric species.

The local prevalence of good carriers or bad ones must depend not only on regional factors but on the general zoological laws which determine diffusion of species.[1]

(12). *The social factor.*—The factor b depends not only on the appetite, energy and enterprise of the mosquitos, but also on the intelligence, social status and habits of their victims. Stupid, poor, lazy people, living in badly-made huts, without much clothing and without mosquito-nets, are sure to be bitten much more easily than more civilised races. People who burn wood or cow-dung in their houses in the evening, or who rub their skins with oil, earth or sandal-wood, or who close their rooms at night, may perhaps be bitten less than others. The subject is too complex for detailed examination here. The habits of man and mosquito are probably often correlated. Thus where the principal carrier is an out-of-doors biter, people who sleep or work at night in the open are likely to suffer. Alcoholism, opium, etc., lead to neglect of precautions. Children are sure to be easy victims. Farm stock and dogs may satisfy many insects, and punkas and fans drive away others.

Neglect of precautions against being bitten is likely, not only to increase the biting factor, but also, by section 29 (7), the total number of mosquitos.

Famine, poverty and other diseases will reduce the recovery factor.

(13). *Possible effect of malaria on the Anophelines.*—As early as 1898 I thought it possible that the parasites might injure their insect hosts, as well as their human ones. If this happens we can readily understand that an epidemic of malaria might tend to limit itself by killing large numbers of the carriers as well as men. Thus, during an epidemic year, so many of the insects might die that the breeding might subsequently be reduced for some time. But there are reasons against this view. I could never satisfy myself that *C. fatigans*, even when

[1] Possibly also on the insects' food (section 48).

extremely heavily infected with *Proteosoma*, died sooner than when not infected at all. Probably they do so, but only to a slight extent. Then again, only a small percentage of infected mosquitos are heavily infected, so that the malaria infection is not likely to make a material difference in their death-rate. Lastly, even a severe epidemic among them will quickly be compensated for by their rapid proliferation.

(14). *Seasonal variation.* — This is a matter of universal observation. As a general rule in the northern hemisphere the disease reaches its *maximum* prevalence in the autumn, say October or November. At that point, a rapid decline, the *winter fall*, generally begins, and this continues for several months until early spring, say February or March, when the *winter minimum* occurs. The *spring rise* now takes place, and the disease tends to increase with more or less regularity until the next autumn maximum. Of course, the seasons are reversed in the southern hemisphere. Innumerable illustrative statistics might be given; but every one is acquainted with the phenomenon, and the examples in section 20 will suffice for our present purpose (note that in the Italian figures the minimum is reached in June).

The causes of seasonal variation will be apparent from a study of the variation formula given at the beginning of this section. Thus the *winter fall* is probably due to diminution of the factors b, s, a, in consequence of the cooler temperature or drying-up of the rains, or of both; and also possibly to increase of the recovery factor owing to the more bracing climate. On the other hand, the *spring rise* is probably due to just the opposite changes in these factors, and possibly also to the emergence of Anophelines which have been hibernating through the winter.

In the tables in section 20 I have given the ratios between the average admissions for successive months. These can be compared with the variation formula, by supposing that m_1 is the malaria ratio for any given month, and m_2 the malaria ratio

for the next month. Then, dividing the variation formula throughout by m_1, we have,

$$m_2/m_1 = 1 + b^2 si \cdot a\,(1 - m_1) - r.$$

Thus, take the monthly averages and ratios of the native troops, and suppose that only the number of men actually infected were admitted (section 31 (5)) into hospital every month. Then, out of 43,330 men there were on the average 922 infected men in February and 1,030 in March. Thus $m_1 = 922/43{,}330$; $m_2 = 1{,}030/43{,}330$; $m_2/m_1 = 1\cdot12$; $1 - m_1 = 0\cdot98$; and therefore,

$$1\cdot12 = 1 + 0\cdot98\, b^2 sia - r.$$

Supposing that b, s, i, r have the values assigned at the beginning of this section, we calculate from this that $a = (0\cdot12 + 0\cdot2)/0\cdot0049 = 65$. That is, as a rough estimate, there may have been about 65 carrying Anophelines *per* person during the month February to March in order to increase the infected persons from 922 to 1,030, and the admissions from 461 to 515.

Similarly, in order to cause the great rise in admissions among the native troops from averages of 1,689 in September to 2,955 in October, we may estimate roughly that 175 Anophelines may have been present. In order to cause the commencement of the winter fall from 2,955 admissions in October to 1,932 in November, we shall have from the formula a negative number for a—that is, according to our data, there should be no Anophelines biting at the time. But of course our data are only speculative ones, and, as already stated, the factors b and s, as well as the Anopheline factor, are likely to vary with season. Moreover, my estimate that the recovery factor r equals about 0·2 is probably too high for the earlier months of infection.

(15). *Annual variation.*—This also is a matter of general observation. In every country there are "good years," "bad years" and "years of epidemic." This variation, when it occurs over large areas, is probably due principally to the influence of

climate, *i.e.*, rainfall, number of rainy days and temperature, on the Anopheline factor.

In the statistics of section 20, the numbers of admissions differ not only for the whole years, but also for the same month in different years, and for the European troops, native troops and prisoners, respectively, during the same year.

It is interesting to note that a considerable rise in the total annual admissions may, by our formulae, be produced by a comparatively small rise in the Anopheline factor. Thus among the native troops there were 11,293 admissions in 1903 against 19,567 in 1901. But by the static formulae the former number would be given by $a=55$ and the latter by $a=70$. It would be almost impossible, for the reasons given in section 29 (4), to detect this increase in the number of Anophelines (28%) by ordinary observation. Hence the unwary observer might assume that the rise in the number of cases was independent of the rise in the number of Anophelines. In fact this is one of the principal stumbling-blocks of students. They observe a considerable annual variation in the number of cases without, *apparently*, any great Anopheline variation. But I doubt whether an Anopheline variation of as much as 50% or even 100% would be easily detectable by the very unsatisfactory methods of counting mosquitos at present known to us. If there are over 40 different Anophelines to each person per month, we should probably be able to find only two or three daily in his house—and possibly less if the insects are "wild" ones. This number might be doubled or trebled without producing a change which would be readily noticed even after careful observation, and long-continued random sampling would be required to establish the variation over a tract of country. Yet, as we have just calculated, a 28% increase in the Anophelines might cause a 42% increase in static malaria—that is, an increase of admissions from 11,293 to 19,567.[1]

While, I think, the annual variation is *generally* due to climatic causes, it is very possible that other factors often

[1] See also section 66.

influence it—such as poverty caused by increase in the cost of food (which would diminish the recovery factor), or movements of the population which might act in various ways.

(16). *Effect of imported cases.*—In section 28 I argued that, whatever the original number of cases may be, the malaria in a locality tends finally to settle down to a fixed static ratio determined by the various constants. Thus if we take the case of a village of 1,000 people, half of whom are infected to begin with, with 60 Anophelines *per* person, the rate would *fall* until, finally, about 333 persons are always infected. If we had started in the same village with the same number of Anophelines, but only one infected person, then the rate would *rise* until the same number of infected persons are found. But the reader must not infer that the original malaria ratio is of *no consequence*—that it does not matter whether many cases are imported or not. True, the ultimate static ratio should be the same, but many months may elapse before this limit is reached. If there have been about 333 cases in the village for months or years past, and we now add 167 imported cases, then (neglecting the small change in the total population) we should have 500, 475, 455, 438 and 424 cases in the four following months; that is, 167, 142, 122, 105 and 91 more cases than there would have been in the village if no imported cases had been introduced. This would probably be looked upon by the villagers as a serious epidemic due to some climatic cause.

On the other hand, suppose that there have been 40 Anophelines *per* person in the village. Then the static malaria there would be zero. But if 500 of the villagers are now replaced by 500 infected persons, the 40 Anophelines, though insufficient to maintain static malaria, would be sufficient to spread the disease to some extent among the villagers; as will be seen by comparing examples 3 and 6 of section 28—that is, the examples with 40 and with 0 Anophelines. In four months the former will have 500, 450, 409, 376, 348

cases, some of them among the healthy villagers; and the latter will have only 500, 400, 320, 256, 205 cases, all among the imported persons. Thus in the former there will be 50, 89, 120, 143 cases among the villagers, who will, of course, suffer from an epidemic. But in the end the epidemic will die out.

In this village then, 40 Anophelines *per* person will suffice to spread the disease to some extent from the imported cases to the healthy villagers, though the various small epidemics which may be caused in this way will tend ultimately to die out. But suppose that owing to the blocking of some stream the Anopheline factor is increased to 60. The imported cases will now not only spread the disease to the healthy villagers, but the epidemic, instead of dying out, will continue to increase until the static limit of one-third of the population is reached. The village, which previously suffered only from sporadic outbreaks due to imported cases, will now become permanently infected. Yet this great change will be due merely to the small increase of Anophelines from 40 to 60 *per* person—an increase which few observers would be able to detect.

It is unnecessary to labour this matter further—I have already laboured it perhaps too much. The point to be understood is that a small Anopheline variation—too small to be easily observed—may yet make a great difference in the local endemicity. Below a certain figure the Anophelines will be, according to the laws of chance, too few to produce enough new infections to keep pace with the natural recoveries. Above that figure they will be sufficient for the purpose, and the malaria ratio will rise to a definite static limit.

So far, this theorem has been already given in section 28; but we now perceive another point of importance. Though the Anophelines are too few to maintain a static endemicity, yet if any at all are present they may suffice (provided that the other factors *b*, *s*, *i*, *r* do not possess *prohibitive* values) to cause small temporary spreadings of the disease from imported

cases among the local population. Only in the case where there are practically no Anophelines will the imported cases have no effect at all. The reader will easily grasp this law by comparing the examples in section 28.

The reader should also remember another point. Our formulae are based upon the calculation of *average* chances. Thus, if the average Anopheline factor is over forty, and a single case of malaria is imported, we argue that the chances are that the disease will spread in the locality. We do not say that the disease must or will spread to a *certainty*. The laws of luck come in here. The distribution of Anophelines cannot be absolutely uniform in any locality. The imported case may come to reside at a spot where there are few Anophelines, so that the chances are that the disease will not spread at all. Or he may reside where the Anophelines are a little more numerous, so that only a small temporary epidemic may be caused. Or, if he happens to live where there are many Anophelines, a permanent rise in the malaria rate may be produced. Yet in all these cases the *average* Anopheline factor for the whole area under consideration may be anything. Thus, again, the imported case may have no gametids in his blood; or he may arrive in winter and recover before the mosquito season; or he may use mosquito-nets. This will be a case of good luck; but by bad luck none of these events may occur, and an epidemic may be started or increased.

We must now consider some special cases of importation.

(17). *Isolated supermalarious or inframalarious areas.*—Suppose the case of a country in which the static malaria is generally low, but in which an isolated area becomes, from any cause, more highly malarious: what will be the effect of that patch of high endemicity upon the health of the surrounding country of low endemicity? Evidently, unless the inhabitants of the supermalarious patch are strictly confined to it, some of them will tend to wander away from it, especially if, as often happens, they are actually driven out of it by its

unhealthiness. If there are any Anophelines at all in the surrounding country, these emigrants, when infected, may cause local spreading, slight, severe, transient or permanent, according to the local factor. In any case they will tend to increase the surrounding endemicity. To calculate the increase from the variation formula, we must add, month by month, the imported cases to the number of cases remaining over from the previous month. Thus, though the static malaria ratio of the surrounding country might be naturally low, the continued importation of cases from the focus of high endemicity might raise it very considerably.

It may be said that this process cannot continue for ever, because the focus would soon become depopulated. This certainly happens in some very unhealthy spots; but in others the population of the focus is maintained by constant immigration, so that it continues to be a perennial source of danger to the neighbours. And its influence may be felt for considerable distances.

Now, consider the converse case—that in which an isolated patch of low endemicity exists in the midst of a larger area of high endemicity. The effect should be just the opposite. There should be obviously less malaria in the whole country than if the inframalarious patch had not existed at all; and the larger the healthy patch, the larger its influence will be.

In other words, we assume that inequalities of malaria distribution have results similar to those of inequalities of heat distribution or mosquito distribution. The superheated objects or supermalarious areas tend to add heat or malaria to the surrounding objects or areas. But I am not clear that this resemblance always holds. Thus the people, and perhaps the Anophelines, in the supermalarious patch may, after a time, become so immune that they will no longer add very much to the surrounding ratios. Conversely, the non-immunes in an inframalarious patch may raise the ratio there owing to "extraneous infection" (19).

(18). *Effect of mass-migrations.*—Suppose that a large

number of persons, with a given malaria ratio, are suddenly and simultaneously moved into a country possessing a different malaria ratio: what will be the effect? Such events frequently happen, as when a regiment of soldiers coming from England or from a very malarious country is moved into a moderately malarious country; or when numbers of coolies are collected in a locality which has a different malaria ratio from that in which they were collected.

The effect will probably be that there will be an epidemic in both cases. An inframalarious regiment of non-immunes from England is sure to suffer to a degree determined by the local factors. Its own men will be the principal ones to suffer, but the numerous fresh cases among them will tend to react on the local population. In the case of the introduction of a supermalarious regiment, the surrounding local population will suffer most, and will in turn react on the men of the regiment. In both cases the malaria ratio will ultimately tend to settle down to the static ratio of the locality. Examples of both of these cases are of frequent occurrence. Large bodies of labourers collected for various kinds of engineering works are liable to cause outbreaks, either among themselves or among the surrounding population, according to the respective endemic ratios and the local factors. This will be especially the case if the aggregation of human beings causes an actual increase in the local Anopheline factor (9). In such a case the aggregation may have all the bad effects of a patch of high endemicity described in the last subsection.

(19). *Extraneous infection.*—By this I mean the infection of people during occasional visits outside their usual habitation—a very important case. There are many villages the malaria ratio of which under natural conditions would be low, but in which it is maintained at an unnaturally high level by the fact that the inhabitants work in more malarious areas at certain times of the year. Thus in the village of Mazi (575 inhabitants), situated several hundred feet above the Plain of

Kopais in Greece, I found malaria infection in twenty out of forty children examined [1906]; but there were scarcely any possible breeding-pools among the broken hills round the village. The apparent anomaly was easily explained by the fact that every year nearly the whole population descends to the plain for the harvesting in August (the most malarious month), and camps there for days or weeks at a time. As the most malarious months are also generally those of the greatest agricultural activity, extraneous infection is probably one of the principal equalisers of malarial distribution. In fact, the result is the same if the factors of endemicity are temporarily raised in a given locality, or if the people temporarily migrate to another locality where the factors of endemicity are higher. The local endemicity depends upon the local factors, only if the local population remains stationary. Of course if there are any local Anophelines at all, persons infected extraneously will tend to spread the disease to some extent among those who do not leave the locality. Thus adults infected while harvesting at a distance may infect their children when they return home.

(20). *Slow changes in endemicity.*—Seasonal, annual and other variations such as we have considered are constantly occurring in every malarious country; but, besides these, certain slow but persistent changes, sometimes affecting very large areas, are often observed, or at least reported. It is stated that a whole country is gradually becoming more and more, or less and less, malarious. As undeniable examples I may quote the entry and increase of malaria in Mauritius, and its disappearance from Britain. How explain the phenomena? They may be due, not to one, but possibly to any or all the factors concerned in the malaria equation. Let us first consider each of the possible factors in turn.

(*a*) *The Anopheline factor*, that is, the number of carrying insects *per* person, may slowly and persistently vary. As shown in (15) a scarcely-appreciable variation in this factor

may produce a striking change in the endemicity. Thus with 60 Anophelines *per* person the static malaria ratio should stand at about one-third; but it should stand at double that with the Anophelines at 120. Yet few persons would be able to observe this difference in the number of the insects; and as no census has been taken in the past, the scientific student would possess no standard for comparing the present with it. Yet many things may occur or concur to influence this factor to a considerable degree.

Thus it is well known that long but inexplicable undulations occur in the *weather*-curves. These often affect only certain tracts of country, and have marked results upon the local crops and prosperity. They may thus quite possibly make a considerable percentage of difference in the Anopheline output, besides tending to influence the recovery factor. It would be difficult to state precisely what the exact effect of the weather might be. Excessive rainfall might either increase or decrease the Anopheline output, especially of certain species, according to local breeding conditions. Thus we should observe the change in the malaria without being able to draw a very clear correlation curve between it and the weather.

Again, according to Walter [1908], afforestation has a marked effect on the number of rainy days. The great exhalation of moisture from vegetation tends to increase the afternoon thunder showers which occur so frequently over tropical lands (but not seas) at certain seasons. It has considerable influence on crops, and doubtless helps to keep breeding-pools filled. Irrigated *cultivation* is evidently likely to have a large similar effect, especially with extensive irrigation works and with certain corps. High-class roads and railways tend to obstruct natural surface drainage and also to provide numerous breeding-pools in the "borrow-pits" along their route (section 57); and I have seen the undoubted effect of them in Sierra Leone, Lagos, Panama and Mauritius.

But whether the increase of total breeding surface produced by them is sufficiently large to modify the average Anopheline factor over a great area is another question. Probably irrigated cultivation has a larger effect. General increase of population may, by section 29 (7), increase the mosquito output out of proportion to the increase of the human population, by supplying more food to the female Anophelines; and diminution of prosperity may have the same result, by (12). Thus many things may modify the Anopheline factor; and as we possess no exact method of measuring mosquitos it will always be difficult to decide how or why the factor has been changed, or if it has been changed at all.

(*b*) Changes in the *carrying* and *biting* factors may occur as the result of a gradual predominance of certain species of Anophelines. As we now know, the fauna and flora of a country frequently change to a considerable extent. Those originally found in ocean islands, like Mauritius and the Seychelles, are apt to be almost swamped by imported species. Quite possibly, therefore, more "virulent" species of Anophelines may gradually increase very greatly in certain areas, at least for a time under certain cycles of weather or with certain kinds of cultivation.

(*c*) Human *immigration* into certain areas, especially in connection with large planting, railway, mining or irrigation works, may tend to create *foci* of malaria (17); to enhance the local rate by the importation of cases or even of non-immunes; to increase the convection factors *s* and *b*, by destitution or bad housing; or to raise the Anopheline output by additional food.

(*d*) The *habits* of the people may change in consequence of prosperity, famines, pestilences or increase of civilisation. Thus they may use better houses, or houses of two storeys, or mosquito-nets, or glass windows. Or a larger proportion of medical men may practise among them; or quinine may

be more accessible; or they may abandon cultivation for trade or take to cities—and so on.

It will always be a very difficult matter to select the most probable factor. The final result will generally be due to the balance of all the forces, some pulling this way and some that. S. R. Christophers and C. A. Bentley have recently [1909] considered that a slow increase in certain Indian areas is due not so much to the Anopheline factor as to (*c*)—that is, immigration in connection with large economic developments. Their paper is thoughtful, but, unfortunately, they do not deal with their subject quantitatively. They give us no estimate of the degree of the reputed epidemics, of the proportion of immigrants and of area occupied by them to total population and area, of their methods of enumerating the Anophelines, and, indeed, of comparing the effects of any of the possible factors. It is evident that immigration and allied factors may have some results, but it is necessary to calculate *how much* compared with those of other influences. Without some attempt at exact quantitative estimates, the scientific study of the diffusion of malaria is as impossible as that of the diffusion of heat. Many similar medical papers on the subject have the same deficiency.

(21). *Examples: Mauritius, Ismailia, Clairfond, Britain.*—In section 1 reasons were given for the opinion that malaria might have been comparatively scarce, or entirely absent, in Greece and Italy before the historic period, and that after about 550 B.C. it probably increased greatly in amount. This theorem can scarcely be proved; but I will now describe briefly the similar events which have occurred within the memory of living persons in Mauritius and Réunion.

The island of *Mauritius*, situated about 550 miles (885 kilometres) east of Madagascar, and lying just within the southern tropic, covers an area of 705 square miles (1,825 square kilometres), rises to a height of about 600 metres in the centre, from which it slopes down more or less gradually

to the sea. The climate is the usual tropical marine climate, warm, equable and humid, and the vegetation is rich. Discovered in 1507, it was peopled by Dutch and then by French settlers, who were always introducing slaves from Africa and Madagascar. In 1810 it was taken by the British. The slaves were set free in 1834, and from next year onward an immense importation of coolie labour from India commenced and continued. Thus more than 20,000 coolies were introduced in each of the years 1843, 1854, 1858 and 1865, and in 1859 no less than 48,377 were imported. Quite certainly large numbers of these immigrants must have been infected with malaria, yet, equally certainly, no endemic malaria occurred in the island all this time. I examined the point with the utmost care for the purposes of my Report to the Government [1908], looked through endless returns and studied the voluminous evidence placed before a Commission of Enquiry. A. Davidson [1892] and many other writers have reached the same conclusion. In Saint-Pierre's classical novel *Paul and Virginia* (1789) no mention is made of malaria in the island. Dr Lorans, the late lamented Medical Director, who always lived there, informed me that there was none when he was a boy. At that time the planters and whites lived in houses scattered all over the country, and they and the British troops and officials lived entirely free from local infection. Relapses of malaria and other fevers such as relapsing fever, were, of course, noted among the immigrants, but the malaria did not spread, though a few medical men suspected the existence of occasional sporadic endemic cases since (?) 1857. In 1865, however, a small outbreak commenced in a marshy district near the capital, Port Louis. Next year the epidemic reached the capital, and in 1866-1867 attained alarming proportions. It was like an epidemic of plague or cholera. One quarter of the inhabitants of Port Louis died from all causes in 1867, and 6,224 out of a population of 87,000 in one month (April). The whole

death-rate of the island rose from 32/1000 in 1866 to 120/1000 in 1867, the total population being 360,000. The disease spread in two years all over the lower parts of the island, and has remained, and probably increased, ever since (section 32). In the sister island of Réunion, 125 miles distant, precisely similar events occurred at the same time. There also there had been no previous endemic malaria in spite of the introduction of slaves from Madagascar and of Indians from India.

How explain this extraordinary and terrible event, which shattered existing notions about malaria? The present carrier is *P. costalis*, an African, not an Indian mosquito. If it had existed in the islands in large numbers the epidemic would have commenced long previously—certainly with the beginning of the copious Indian immigration, thirty years before its actual commencement. If it had existed in small numbers, below the endemic limit, it ought still to have spread the disease to some extent, according to the reasoning in (16); but most authorities denied that this occurred at all before 1865. Moreover, on both these suppositions, the chances are that the epidemic would not have commenced simultaneously in both islands, especially because the Indian immigration into Mauritius was much greater than that into Réunion. I can only conclude, therefore, that *P. costalis* was introduced from Africa or Madagascar simultaneously into both islands, probably by the same vessel, a year or two before the epidemic; that it spread rapidly and soon multiplied over the endemic limit; and that from this point the success and ultimate permanence of the malarial invasion was assured. Of course the inhabitants would not be able to distinguish the new-comer among the swarms of innocuous mosquitos already present, and there appears to be no reason why, after being once introduced, it should not have reached the endemic limit of numbers in a year or two. This therefore appears to be wholly a case of the Anopheline factor. The

human factors had been present for at least thirty years without causing the disease.

A similar case is that of *Ismailia* (section 53). Situated on the Suez Canal in the desert, it remained free from malaria until the fresh-water canal was completed in 1877, when malaria appeared almost immediately. Since then the disease continued to increase with fluctuations, until it was banished in 1902 by proper mosquito reduction. Here again the outbreak was probably due almost entirely to the Anopheline factor.

A more difficult case is that of an outbreak in the neighbourhood of *Clairfond Marsh* in Mauritius (section 30 (1) map), already frequently mentioned. This marsh existed on a plain 1,400 feet (427 metres) above sea-level, and the epidemic is described in my Report [1908] by Dr de Chazal who had been practising in the neighbourhood since 1890, and by myself, and also by Major Fowler [1908]. The history is as follows :—

Before 1903 Dr de Chazal, who lived not far from the marsh, had observed a few cases of malaria, but only among Indians. In that year, however, cases began to occur among well-to-do people living in the neighbourhood, and the epidemic now developed rapidly. The cases at the neighbouring dispensary were as follows :—

1904	1905	1906	1907
346	843	1,147	1,487

and similar rises in the cases of malaria at seven other dispensaries, situated not far away, also occurred. In 1906 the British soldiers in barracks (see subsection 1) began to be affected. In 1908 Major Fowler and I witnessed an epidemic of seventy-one cases in January among these troops, and on making a house-to-house visitation, found a spleen rate of $241/339 = 71\%$, and an average spleen of 4·12 among the Indian children living round the marsh. The disease was being carried by *P. costalis*, which our "moustiquiers" found

with considerable difficulty, and not by *M. mauritianus* which abounded round the marsh. The marsh was now drained by the Government; quinine was simultaneously distributed in the houses by ourselves and a dispenser; and the epidemic ceased.

How explain it? The marsh had been there as long as any one could remember, and *P. costalis*, though it prefers sea-level, had probably been breeding in it in small numbers all the time. Indians as well as the richer classes had been living near at hand, and imported cases had certainly been common. Why, then, did not the epidemic commence before? Up to 1902 the population round the marsh had been comparatively scarce, but in that year a great increase of Indians took place owing to the advent of a regiment of Sepoys, and many of these Indians built huts close to the marsh waters. Here, then, might be a case of creation of a focus of high endemicity; but still I find it difficult to explain the focus without supposing an increase in *P. costalis*, *pari passu* with that of the human population. The case is most probably explained by an increase due to more abundant food in the new huts built close to the marsh, according to sections 29 (7) and 30 (9 and 12); and possibly by an increase in the biting factor, due to the growth of the village nearer to the marsh.

The disappearance of malaria from *Britain* has been ably described by G. H. F. Nuttall, L. Cobbett and T. Strangeways-Pigg [1901]. The observers made a careful study of the former distribution of the disease in Britain, and of the present distribution of the Anophelines. The two coincided to some extent—that is, the malaria was within the Anopheline area, but not so extensive. The disease, formerly severe, began to decline early last century, but still lingered on until about 1850. I was "clinical clerk" in St Bartholomew's Hospital in 1879 to a woman with greatly enlarged spleen and characteristic fever, who lived in the Fen Country and had never been out of England. Since that time endemic malaria has entirely

vanished from Britain, with the exception of two cases among soldiers (footnote, section 14).[1]

The three British Anophelines are *A. maculipennis*, *A. bifurcatus* and *A. nigripes*. They are all capable of carrying malaria, but Theobald thinks that the first will not bite man in Britain. The authors conclude that the disappearance of malaria has been due (*a*) to reduction of Anophelines by drainage; (*b*) to reduction of population by emigration; and (*c*) to the use of quinine. I think that (*b*) is not sound. The disappearance is almost certainly due to the reduction of the Anopheline factor by drainage below the endemic limit, even in the Fen Country, and also to enhancement of the recovery factor by the more general use of quinine. I wonder, however, whether there has been any change in climate, and also whether glass windows have been used more since the beginning of last century.

Many other examples might be cited, but the reader will doubtless prefer to consider them for himself.

(22). *Possible errors in the study of endemicity.*—One is very apt to find the local endemicity too high in comparison with the local number of Anophelines to be found in houses. In 1897, in an intensely infected plantation near Ootacamund, India, I found during several visits only one Anopheline (the first I ever saw), although I searched the houses myself and offered the people rewards—and this was at the height of the malaria season. In fact there were so few mosquitos of any kind that I began to doubt the whole mosquito theory (just before the discovery of the zygotes). The local carriers were probably "wild" mosquitos which I did not know how to look for; or else I may have missed the principal hatching-out season. At Clairfond ten trained "moustiquiers" could procure only a few *P. costalis* every night. We must never assume,

[1] Sir Clifford Allbutt tells me that he saw numerous cases in Cambridge in 1860. Nuttall, Cobbett and Strangeways-Pigg verified a case at Norwich in 1898 [1901, p. 31].

without clear proof, that the number of insects seen or caught during a few hours' or days' search affords any exact measure of the number actually present during that period—still less of the number which may be present at other periods.

It is often observed that the malaria ratio is high where there appears to be little breeding-surface—that "dry" places seem often to be more malarious than "wet" ones. But we have no right to assume that the carriers always breed more in the deep or permanent pools which constitute "water-logging" than in the shallow, evanescent rain-pools which are often the only waters found in the dryer areas. The latter may perhaps give a much larger output at the proper season than the former can yield all the year round (sections 29 (8) and 30 (1)).

In some cases the Anopheline factor is probably truly below what we might expect from the malaria ratio; but such cases may be due to extraneous infection (19), or to immigration from neighbouring areas of high endemicity (16, 17).

Statements are frequently made to the effect that though the number of Anophelines in a locality is large the malaria ratio is small—as, for instance, the case reported by Celli [1902]. Here again we must not assume that the number of Anophelines actually found during a few days is an exact measure of those existing all the year round. We have, perhaps, done the counting just at the moment when the insects happened to abound. Or, on comparing two localities, we may have been concerned with a domestic species or variety in the locality where the insects appeared to be more numerous, and a wild species or variety in the other locality. Even where it is proved by continued observation that the Anophelines do really average a high figure where there is little malaria, this will merely suggest that some of the other factors are unsuitable; for example, the local insects may belong to an immune strain. Thus we have never succeeded in infecting the *A. maculipennis* round Liverpool from infected sailors.

In fact those who pretend to study the correlation between the Anopheline factor and the malaria rate are very prone to make two mistakes: (a) they often fail to consider deeply enough the principles involved, and (b) they are apt to base startling "discoveries" on quite insufficient random-sampling—an error into which, according to Professor Karl Pearson, medical men frequently fall. So far as the human reason enables us to judge, there must be some correlation between the two quantities. When, therefore, we are told—as we frequently are, especially in India — that so-and-so has not observed any such correlation, we naturally look for the detailed figures of his investigations; and when we find, in place of these, mere statements of opinions and impressions, we naturally infer that it is not the principles which are worthless, but the observations. A person who thinks to settle such difficult questions merely by running from village to village, catching a few Anophelines and examining a few people, can have little knowledge of the nature of scientific evidence. To yield any worthy results at all, the enquiry would demand the most careful and continuous study of all the factors concerned (sections 29 (4), and 31).

A mistake frequently made is to suppose that there must be some correlation between the number of Anophelines existing on a given day and the total percentage of infected persons. But the former will tend to influence only the *inoculation rate* of the *future*. The total percentage of infected persons will depend upon *past* factors which may no longer exist. They are often, or generally, connected, but not necessarily always so.

Equal errors may be made regarding the measurement of human malaria. For example, the disease is often supposed to be specially prevalent among soldiers, coolies or prisoners; but this may be merely a statistical error, due to the fact that these classes are under more careful supervision than the general public is.

(23). I have attempted to examine as carefully as possible

the principal factors concerned in the spread of malaria, but may have overlooked several of importance. The effect of immunity, both among men and Anophelines, deserves much closer study. So also do many questions connected with difference of species of the parasites. It is by no means certain that immunity against one species confers much or any against another. If this is the case, many epidemics may be caused merely by the entry of a species not hitherto prevalent in a locality; and the simultaneous prevalance of all three species may lead to a much more serious fever rate. Koch observed [1900] that comparative immunity against quartan seems to have little effect against the other parasites; but I am not sure that reaction against an existent invasion in a person will have *very much* effect against a new infection even by the same species.

The effect on the local malaria rate of *desertion* of marshy areas (9) should be similar to that of drainage, and must not be forgotten though it is not often witnessed. Conversely, the effect of *approximation* to a marsh, as when people come and settle near one (for example, the Clairfond outbreak just described), should be similar to the effect of increase of marsh. Obviously it will come to the same thing if we bring a marsh close to human habitations, or bring human habitations close to a marsh.

31. The Measurement of Malaria.—In section 26 I gave definitions of the terms *malaria rate*, *endemicity*, and *inoculation rate*. We have now to consider how these and other estimates can best be obtained for a given population inhabiting a given area. Note to begin with that we can never obtain any such rates exactly; and also that the degree of approximation to the truth must always depend on the amount of time and labour we have to spare for the task.

Our estimates may be of two kinds, *actual* or *comparative*. *Comparative* estimates are those which seek to compare the

proportion of infected persons in the same locality at different times, or in different localities at the same time. It may often be much easier to obtain comparative than actual estimates.

Let us consider first the different possible methods, and then the respective values of them.

(1). *The parasite rate and index.*—By malaria rate or ratio I mean the percentage or proportion of people who really contain living plasmodia at some given moment—not those who show symptoms. By *parasite rate* I mean the same thing. Now in section 18 (8) and (9) it was shown that an average man contained about 3,000,000 c.mm. of blood, and that a quarter of an hour is required to examine 1/50 of a c.mm. under the microscope. Thus in that time we can search only 1/150,000,000th of a man's blood. In other words, if there are less than 150,000,000 parasites in the patient's blood the chances are that we may not find one, even after a quarter of an hour's search. Hence we can never hope to ascertain with certainty whether a single patient does or does not contain plasmodia, and can therefore certainly never discover the actual parasite rate of any number of people.

By *parasite index* I mean the percentage of persons in whose blood plasmodia are found after fifteen minutes'[1] search—that is, of persons who contain more than about 50 plasmodia to 1 c.mm. To ascertain even this is a laborious task, requiring for 100 people twenty-five hours' continuous work.

Obviously the parasite index must always be less than the parasite rate. What ratio will the latter bear to the former? We do not know. Out of 100 infected persons perhaps only 60 or 70 may contain more than 50 plasmodia per 1 c.mm. of blood at a given moment. But this is a mere guess, and no accurate studies have been made on the point. The ratio certainly varies largely, but as we do not know it with certainty, the parasite index gives no exact information

[1] Some authors are satisfied with five or ten minutes' search.

regarding the *actual* parasite rate. It gives valuable information, however, on one point. Every person in whom parasites are found is certainly infected. Hence the parasite index gives at least the *minimum possible* percentage of infected persons—that is, *we know that the actual parasite rate cannot be lower than the parasite index.*

For comparative estimates the parasite index is still more useful, because in them we deal only with ratios from time to time or from place to place. Thus if it has varied from say 20% to 55% in two large aggregates of persons examined, we may infer that the actual parasite rate has varied to a similar amount. We do not know what that actual parasite rate was, with the parasite index either of 20% or of 55%, but we conclude that its variation has been, *ceteris paribus*, similar—that is, from 20% to 55%.

As originally observed by Koch and Stephens and Christophers, confirmed by Ziemann, Plehn, Annett, and Dutton and many workers, the parasite index varies greatly according to *age*, being generally highest in children (9). Hence comparative estimates must be based on a study of persons of the same age. *Race*, *quinine treatment*, and *social status* seem also to have considerable effect.

The method is open to several sources of fallacy. The *skill* of the observer may make a difference of say 20% or more in the results obtained; and the *time* employed in the search for plasmodia is very important. In 1906 I obtained a number of slides of blood from Greek children, and since then these slides have frequently been given for study to the classes of the Liverpool School of Tropical Medicine. On every occasion that this has been done, one or more of the students have discovered parasites in specimens in which they had not been previously found! In making exact comparative estimates it is therefore necessary to employ observers of the same skill, and to fix the time during which the search must be made.

But the principal weakness of the method is due to the fact that, owing to the labour involved, only a comparatively small number of people can be examined—so that the error of random sampling is likely to be very large. Thus, suppose that we have examined 50 specimens for 15 minutes each, costing 12·5 hours' work (exclusive of collecting and staining), and have found plasmodia in 25 specimens, we have no right to infer that the parasite index of all the people in the locality will be 50%. The chances are, by Poisson's formula (8), that if we examine another 50 persons we may find parasites in any proportion of them from 30% to 70%—the error being 20%. Thus, if the same observer, using precisely the same methods, examines 50 different persons on two occasions, and finds 25 infected on the first occasion and 35 infected on the second occasion, he still has no right to assume that the actual parasite rate (*i.e.*, the malaria rate) of the locality has risen between the two examinations.

The method is probably capable of much improvement by quicker means of microscopic diagnosis (sections 65 (1)).

(2). *The spleen rate and index.*—This method has been in use for a long time. We know that the spleen enlarges sufficiently to be detected by palpation in a considerable proportion of infected persons. Such enlargement is discoverable with certainty and in a few seconds by the fingers pressed under the ribs of the left side, and any one—hospital assistants, nurses and laity—can detect it. The persons to be examined are passed in a line before the examiner, while another person records the results; and with good management 100 people can be thus inspected in an hour. Or else we can do the work by house-to-house inspection. In section 22 I gave the results of a "spleen census" of 31,022 children in Mauritius, and of 92,258 children in Ceylon—so that very large numbers of people can be studied by this method. It is open to the following defects:—

(*a*) The enlargement may be so slight in a small proportion

of cases, especially early infections, that it may be overlooked in them if we use palpation only. Generally, however, there is fever, or a history of recent fever, in such cases.

(*b*) The spleen of healthy infants is sometimes so easily palpable that the unskilled observer may think that it is enlarged.

(*c*) Not all infected persons show palpable splenomegaly.

(*d*) Not all splenomegalous persons are necessarily infected.

(*e*) Other diseases, such as kala-azar and various anaemias, cause splenomegaly (section 22); but the former is limited to certain tracts of country, and the latter are too scarce to affect large statistics. Generally speaking, widespread splenomegaly is due to malaria.

The advantages of the method are:—

(*a*) That the enlargement can be detected, practically with certainty and in a few seconds, by almost any one.

(*b*) That the method can be applied with little trouble to enormous numbers of people, thus practically avoiding the error of random sampling.

I have never known *rupture* of *spleen* to follow palpation; but the possibility of this must be remembered.

We must note that a smaller degree of splenic enlargement can be detected by *percussion* (as practised by medical men) than merely by *palpation*. By the former method we can nearly always detect *some* enlargement in malaria. Thayer [1898] claims that it always occurs. Mannaberg says [1905] that out of 132 cases of different types, "cachexias omitted," he found it in all but one case—in 15 cases (11·4% of total) by percussion alone, and in 116 (88%) by palpation also. Thus the finer art of percussion added nearly one-eighth more to the number of cases disclosed by palpation. Laveran agrees as to the frequency of this symptom, but other authors give lower

percentages. It is probably often a question of care in examination. Among sailors treated at the Royal Southern Hospital of Liverpool, palpable enlargement is not very frequent. Many writers give percentages without actuals—that is, nearly worthless figures. L. M. Hope [1904], in a study of 1784 cases, microscopically verified at Pabna, Northern Bengal, states that in 374, or 20%, the spleen was not palpable; but she does not state whether the remaining cases were studied by percussion. In 102 Greek children examined by me (4), the spleen was palpable in 48, and parasites were found in 12 which showed no enlargement (by palpation only). Probably, careful percussion would have disclosed some enlargement in all of these cases. On the whole, then, I think we should conclude that *some degree* of splenic enlargement probably exists in 95-100% of all persons infected with malaria; but that the enlargement is *great enough to be palpable* only in about 75-90%.

The reader should note this difference between enlargements detectable by palpation or by percussion only. In public health work, which demands the examination of large numbers of people in order to avoid error of random sampling, careful percussion is not generally practicable, or, at least, practised.

Thus by the term splenic *index*, I generally mean the percentage of persons in whom enlargement is detectable by palpation only.

In order to determine the spleen *rate* accurately, it is necessary to use careful percussion on all persons in whom no enlargement has been found by palpation. If such persons are numerous this may add considerably to the time required for the investigation—say two or three minutes for each person.

Another important point has to be considered. We have no right to assume that the parasites are still present in all the cases of enlarged spleen, especially in the older cases with large chronic splenomegaly. Such cases may have become parasite-free by establishment of natural immunity; but we can never know in how many the parasites have absolutely vanished, or

have only declined in numbers below the limit at which we can readily find them.

In what proportion of persons with enlarged spleen are there no plasmodia at all? It is impossible to say; but we may give 20% as a rough conjectural estimate.

Suppose, then, that 20% of persons with parasites have no palpable enlargement of the spleen, and that 20% of those with palpable enlargement of the spleen have no parasites—or, at any rate, that the two percentages, whatever they may be, are about equal. It then follows that the palpable spleen rate, or splenic index, will give a nearly exact measure of the percentage of infected persons—that is, of the actual malaria rate of the locality. But at present this theorem, which would be one of great practical importance, is based only on conjectural estimates.

Lastly, we must note that the spleen index is probably modified considerably by local conditions, quinine, and the age of the people examined (sections 22 and 31 (9)).

(3). *Combined parasite index and spleen index.*—If we have time we can use the parasite index and spleen index combined. The people should be divided into four classes: (*a*) those without enlarged spleen or detectable parasites; (*b*) those with both; and (*c*) and (*d*) those with one but not the other. Thus out of 102 children examined in Greece, we found :—

	Parasites only.	Spleen only.	Parasites and spleen.	Nil.	TOTAL.
Moulki . . .	5	23	12	22	62
Mazi	7	4	9	20	40
TOTAL .	12	27	21	42	102

Now if we use the term *endemic index* (section 26) to denote the percentage of persons in whom malaria is diagnosed by *any method*, then the endemic index of Moulki at that time was 40/62 = 64·5%, and of Mazi was 20/40 = 50%. Provided that we know that the splenomegaly is due only to malaria, and also have time to work out the parasite index, this combined method

is obviously more exact than either method by itself. Thus in both these villages together, the parasite index by itself was only 33/102 = 32%, and the spleen index by itself only 48/102 = 47%; while the combined methods give a general endemic index of 60/102 = 59%.

It is important to note that in this example, while the cases with enlarged spleen numbered 48, those with parasites, but without enlarged spleen, numbered 12, or 1/4 of the spleen cases. If this proportion applies generally, the endemic index will be found by adding 25% to the spleen cases. (This figure is, however, too high by the results of Mannaberg and Hope just quoted.)

Again, the cases with parasites numbered 33, while those with spleen but without parasites numbered 27, or 9/11 of the parasite cases. If this proportion holds generally, the endemic index will be found by adding 82% to the parasite cases.

It would thus seem that the spleen index is much nearer the truth than the parasite index—provided that the splenomegaly is really due only to the malaria.

Lastly, we observe that the figures both at Moulki and at Mazi are too small to enable us to make exact comparative estimates of the malaria rate of each village; and we should probably have done better if we had abandoned the parasite index and had spent the time at our disposal in examining the spleen of every person in the villages. Moulki, on the plain of Kopais, contained 350 people, and Mazi, several hundred feet above the plain, contained 575 people. The figures were obtained in May to June [1906], at the beginning of the malaria season.

The number of persons *with parasites but without splenomegaly* seems to me an important figure, because such cases are probably due to recent infections, before the spleen has had time to become enlarged—especially in children. There were no less than 12/102 = 11·8% of such in these two villages,

suggesting that the spring infections had already commenced. The weather was hot, and *A. maculipennis* plentiful.

(4). *Average spleen and average enlarged spleen*.—By the parasite index and spleen index, or by both combined, we attempt to estimate the percentage of infected persons in a locality; but further information can be obtained by recording the degree of enlargement found. For this purpose I suggested in Mauritius [1908] a figure which I call the *average spleen* (section 22). The observer notes whether the size of the spleen, as roughly estimated by palpation, is unity, or about three times, six times or nine times the normal size — these sizes being called normal, or small, medium or great enlargements. He then multiplies the number of persons found to possess each class of enlargement by 1, 3, 6, or 9 as the case may be, adds the products together, and divides by the total number of persons examined. Thus in Mauritius we found, out of 30,137 children of fifteen years and under,

Normal spleens	19,711
Small enlargements	4,381
Medium enlargements	3,479
Great enlargements	2,566
TOTAL	30,137

Thus the average spleen for children in the island works out at 2·54 times the normal size.

Another figure, which I call the *average enlarged spleen*, is found in the same manner, but without considering the normal spleens; that is, multiply the small enlargements by 3, the medium enlargements by 6, the great enlargements by 9, add the results, and divide by the total number with enlarged spleen. In the above example, the *average enlarged spleen* was 5·48. Obviously, according to the rough standards proposed, this figure must always lie between 3 and 9.

These estimates require a little more time to obtain than the simple spleen index, and should generally be attempted only by medical men. I think that one minute a person will

suffice. The information given is more detailed, but also more open to error, since different observers may have different ideas as to what constitute small, medium or great enlargement respectively.

The degree of enlargement probably depends considerably upon the factors given in section 22—racial, social and climatic factors, and age and treatment of the sick. That is, two populations with the same actual proportion of infected persons may have different average enlarged spleens, the difference depending on these factors.

The average spleen depends, not only on the degree of enlargement, but also on the proportion of infected persons (general malaria rate), and may be looked upon as an index of the average amount of illness caused by malaria in the community. The average enlarged spleen indicates only the degree of enlargement where it exists. Several useful deductions may be drawn from the figures. Thus numerous small enlargements would tend to indicate many recent infections; and numerous large ones with few small ones, a past epidemic. The comparative figures for Mauritius may be taken for a standard, as malaria has prevailed constantly there since 1866.

The species of parasite may have considerable influence on the degrees of enlargement. C. F. Craig [1909] thinks that the malignant parasites produce most enlargement; but L. M. Hope [1904] found that in 43 great enlargements malignant parasites occurred in 10, benign tertian in 9, and quartan in 32 (some of the infections being mixed).

The study of the splenic enlargement, so important for public health work, has been curiously neglected in medical literature.

(5). *The fever rate and index.*—By *fever rate* (daily, monthly, annual) I mean the percentage of persons who really have malarial fever during a given period; by *fever index*, I mean the percentage of those who have been ascertained to have it.

The index must be always lower than the *rate*. Even

with soldiers, gaol prisoners and indentured coolies the most careful supervision would be required to detect all the slighter cases. Among troops the bad custom still remains of entering in the books only the more severe cases—which are said to be admitted to hospital, while the slighter cases are only *detained.* This quite falsifies the malaria statistics, and is due only to bad administration. Besides this, many soldiers disdain to come to hospital for slight attacks. If this is the case among these classes, then we can imagine how difficult it would be to estimate the fever rate among a large civil population.

If we could do so, however, we should have a fairly exact measure of the malaria rate — since every infected person probably suffers more or less from illness during at least some part of his infection. But we have no measure of the ratio between the rate and the index. Probably the former is often two or three times the latter, even in the case of troops.

But, while it gives little exact information regarding the actual malaria rate, the fever index is still useful. The *admissions* into military hospitals, civil hospitals and hospitals of gaols and plantations, are generally fairly correctly diagnosed by medical men, and give some idea of the great amount of sickness caused by the disease. The same is true of the large number of *attendances* for malaria at out-patient departments and dispensaries. The figures are valuable for *comparative* estimates of variation from time to time or place to place.

Both for admissions and attendances, the actual numbers of patients admitted or attending any hospital are not very useful, as they may be influenced by the popularity or accessibility of the institution; but the ratio of admissions or attendances for malaria, compared with those for all causes, is always important, not only for comparative estimates, but because it shows the relative importance of malaria compared with all other diseases. It gives us some basis for fixing the proportion of the public funds which should be expended on

the prevention of the former, according to the general sanitary principle enunciated in section 38 (4), namely, that that proportion should depend on the relative importance of the various diseases.

For example, in Mauritius during the seven years 1900-1906 we had the following figures:—

Average population	384,676 a year.
Average admissions for all causes . .	18,761 „
Average admissions for malaria alone .	4,348 „

Thus malaria caused nearly one-quarter (23·2%) of the total *admissions.* Moreover, in the year 1907 alone there were 79.053 *attendances* for all causes at all the hospitals and dispensaries in the island, and of these 28,294, or 35·8%, were reported as due to malaria.

This is an example of the general law that the attendances for malaria are greater than the admissions (in comparison to total attendances and admissions). Obviously only the more severe cases of fever seek admission into hospital, and a much larger proportion of all malaria cases content themselves with mere attendance for advice and medicine.

In India in 1901 (before quinine was much given), out of 300,847 troops and prisoners 108,860, or 36·2%, were admitted for malarial fevers; and these admissions constituted, according to the official figures, 37% of the admissions for all causes. I take this year at random; and it will be seen on inspection of similar statistics in most tropical countries that similar figures hold roughly from place to place and time to time in most of them.

As a very broad general rule we may state that in most, or at least many, malarious countries about one-third of the total population considered require medical treatment for malaria every year; and also that malaria is responsible for about one-third of the total attendances at hospitals and dispensaries. Or, to be still more general, we may put both these ratios at something between one-quarter and one-half.

Of course, error in diagnosis vitiates the figures to some extent. Thus many cases of fever are not malarious at all, but may simply be classed under that heading by careless or overworked observers. The symptoms of the disease are, however, so well marked that I doubt whether the total error produced in this manner is really very large—say over 5%. On the other hand, the error of random sampling for the purpose of obtaining a parasite index is often very much larger.

M. Watson (section 57) has used hospital statistics well for showing the comparative fall of the malaria rate in drained areas. While the admissions from the drained areas fell rapidly, those from the surrounding undrained territories continued as before.

The clinical thermometer may be used more than it is for estimating the endemic index. It could easily be employed together with examination of the blood and the spleen.

(6). *The constantly-sick rate and index.*—By these terms we mean the average proportion of people who are actually ill with malarial fever at a given moment—or who are found to be ill (index). Thus in India in 1901, out of 60,838 European troops, on the average 666, or 1·09%, were always ill with malaria — that is, almost the effective strength of a whole regiment. In Mauritius, Dr Bolton, Medical Officer of the Immigration Department, estimated for me that during the malaria season in the most malarious sugar plantations as many as 20% of the total male adult population may be sick from malaria alone on one day. As the malaria season is often also the busiest agricultural season, such a figure means an enormous loss of labour.

These estimates are important because they afford some measure of the *economic loss* due to the disease. Thus in India in 1901 the European troops lost $666 \times 365 = 243{,}090$ working days during the year, besides loss due to death and invaliding, and loss of capacity for war. But as a rule the estimates can

be obtained only for troops, coolies of factories and plantations, and prisoners. For the general population we can have no definite figures, and can only calculate them on the basis of the former classes.

The *average duration of a case* can be obtained from the constantly-sick index (or *vice versa*) from the following formula:—

Admissions × *average duration* = *constantly sick* × *days in year.*

In India the average duration of a case of intermittent fever among European troops is about two weeks, and of remittent fever about four weeks; but this, of course, means only the residence of the men in hospital, and takes no note of relapses. The figures will be found in Table IV of the Indian statistics.

(7). *Malarial death-rates.*—In most malarious countries medical certificates giving the cause of death are not demanded. Hence in such countries the public statistics are not reliable as regards the cause of the mortality—that is, we can have no confidence in the number of deaths attributed to malaria in the tables. We can often, however, obtain valuable information by calculation from the figures of general mortality; but this subject requires to be dealt with separately (section 32).

(8). *Random sampling.*—Nothing has caused more mistakes in the literature of malaria, and indeed of most medical and sanitary subjects, than ignorance of the great error in reasoning which may be caused by insufficient random sampling.

Suppose that we desire to ascertain some general law regarding a large number of objects. Then the only *exact* way to ascertain the law would be to examine *all* the objects. Suppose, however, that we have no time to do this; then we must content ourselves with the attempt to ascertain the law approximately by examining *samples.* We must take the samples quite at *random*, and must examine *enough* of them. Obviously, if the total number of objects is large, we shall obtain no correct deduction by examining only one, two,

three . . . of them. The more of them we examine the nearer we shall come to the exact truth.

Suppose, for example, that we wish to ascertain the proportion of leucocytes in a patient which belong to the mononuclear variety. We may be very far from the truth if we examine only two or three leucocytes; nearer if we examine twenty or thirty, or two or three hundred, and still nearer if we examine two or three thousand; but we can obtain the exact truth with certainty only if we examine all the leucocytes in his body.

So also if we wish to know the parasite index in a locality —that is, the percentage of people in whom plasmodia can be found after, say, quarter of an hour's search. If we examine only two or three persons we may be very far from the truth; we shall come nearer and nearer to the truth by examining more and more of the people; but we can obtain the exact truth with certainty only by examining every person in the locality.

If we have time to examine only some of the people we can obtain only an approximation to the truth. It is a gross error in science, and yet one which is very frequently made, to suppose that this approximation is the *truth itself.*

Take for example the case mentioned in (1) above. Suppose that there are 10,000 people in a place, and that out of 50 people examined we have found the parasites in 25. Then we are quite truthful when we say that the parasite index of the 50 *people examined* is 50%; but if we presume to argue, on the strength of this small random sampling, that the parasite index of the *whole locality*, containing 10,000 people, is 50%, we make a gross scientific error, and say what may be absolutely untrue—what, in fact, may be not even near the truth. Similarly, if we try to estimate the number of Anophelines in this locality by counting them in only a few houses we may make another error equally gross. Lastly, if, after making two such errors, we next proceed to argue that

there was no relation between the parasite index and the number of Anophelines in the locality, we may perhaps come to a wrong conclusion; but, whether our conclusion is right or wrong, we shall certainly convict ourselves—well, not exactly of scientific competence!

This error is frequently made tacitly in medical literature by recording only percentages, without giving the actual number of objects examined. For example, an observer says that 40% of a patient's leucocytes were mononuclears, or that 20% of the people in a place showed plasmodia, without stating how many total leucocytes or people were examined. Such statements have no meaning whatever—except that the man who makes them is ignorant of this simple law of reasoning. A reader cannot judge the value of a writer's deductions unless he knows the actual facts from which those deductions were drawn. Where a writer merely gives a percentage in this manner, we cannot be sure whether he has not deduced it from a study of only a few objects. But we are sure of something else, namely, that he is not acquainted with the methods of accurate reasoning, and are therefore forced to doubt the value of all his deductions. It is necessary to make these somewhat severe remarks, if only in the hope of saving the waste, occasioned by this common mistake, of otherwise excellent work.

The reader will now remark that it is impossible to examine all the leucocytes in a patient's body, or all the people in a large district. How many must we examine in order to approach sufficiently near to the truth? This question belongs to the domain of statistical science and cannot be discussed at length here; but it is advisable to give the well-known Poisson formula for statistical error, with an addition made by Professor Karl Pearson, the amended formula being particularly useful for the measurement of malaria.

Let N be the total number of objects, such as all the leucocytes in a patient's body or all the people in the locality

under consideration. Let n be the number of these actually examined (say 200 leucocytes or persons); and let p be the number of these found during the examination to belong to a special class—suppose that $p=100$ mononuclear leucocytes or persons found to contain plasmodia. Then p/n will be the corresponding ratio among the objects *examined*—that is, it will be $1/2$, or 50%, in the examples referred to. But we have no right to infer that the same ratio will hold for all the N objects under consideration. Let e denote what is called the error, and $e\%$ the *percentage of error.* Then by the Poisson-Pearson formula,

$$e\% = \frac{200}{n}\sqrt{\frac{2p(n-p)}{n}}\sqrt{1-\frac{n-1}{N-1}}.$$

Examining this formula, we shall see that when $n=N$, that is, when we examine all the objects under consideration, the factor $1-(n-1)/(N-1)=0$, and the whole error vanishes; that is, we obtain the exact truth. But if n is small compared with N, this factor, and therefore the error, becomes larger. If N is very great, as when we are considering all the leucocytes in a patient or all the people in a large district, the same factor approximates to unity, making the error large—unless n can be correspondingly increased.

Thus if, out of 200 leucocytes examined, 100 are found to be mononuclears, then, since N is very large, $(n-1)/(N-1)=0$; so that $e\% = \sqrt{100} = 10$. That is, the error is 10%. We might have inferred, after finding 100 mononuclears out of 200 leucocytes examined, that the same proportion (50% mononuclears) would have held for all the leucocytes in the patient's body. But we have no justification for this assumption. The probability is that if we examine another 200 leucocytes, the proportion in this second batch may be anything between $50\% + 10\%$ and $50\% - 10\%$, that is, may vary from 60% to 40%. All that we can infer, then, from the small sample of leucocytes examined by us, as to the exact truth regarding all the leucocytes in

the patient, is that the percentage of mononuclears probably lies between 60% and 40%—a very different thing from the 50% which we might, ignorantly, have assumed to be the proportion.

Now suppose that we have to deal with a village of 800 people, and that we have examined 200 for plasmodia and have found these in 100 of them. Here the factor $1-(n-1)/(N-1)$ = about 3/4, and the other factors = 10 as before. Hence $e\% = 5\sqrt{3} = 8{\cdot}65$. Thus, while the parasite index of the 200 people actually examined is 50%, that of the 800 people in the village may be anything from 50 + 8·65% to 50 − 8·65%; that is, from 58·65% to 41·35%.

Suppose, however, that in the same village we had examined only 40 persons (as a recent Indian writer recommends) and had found plasmodia in 20. Then the percentage of error $= 5\sqrt{19} = 21{\cdot}8$, and the parasite index of the village will not be 50%, as the unwary student might imagine, but anything between 71·8% and 28·2%—a very ambiguous result. Yet we are often asked to accept important theories on figures like these.

The following square roots may be useful :—

Square root of	2 = 1·41	Square root of	12 = 3·46
,,	3 = 1·73	,,	13 = 3·60
,,	5 = 2·24	,,	14 = 3·74
,,	6 = 2·45	,,	15 = 3·87
,,	7 = 2·64	,,	17 = 4·12
,,	8 = 2·83	,,	18 = 4·24
,,	10 = 3·16	,,	19 = 4·36
,,	11 = 3·32	,,	20 = 4·47

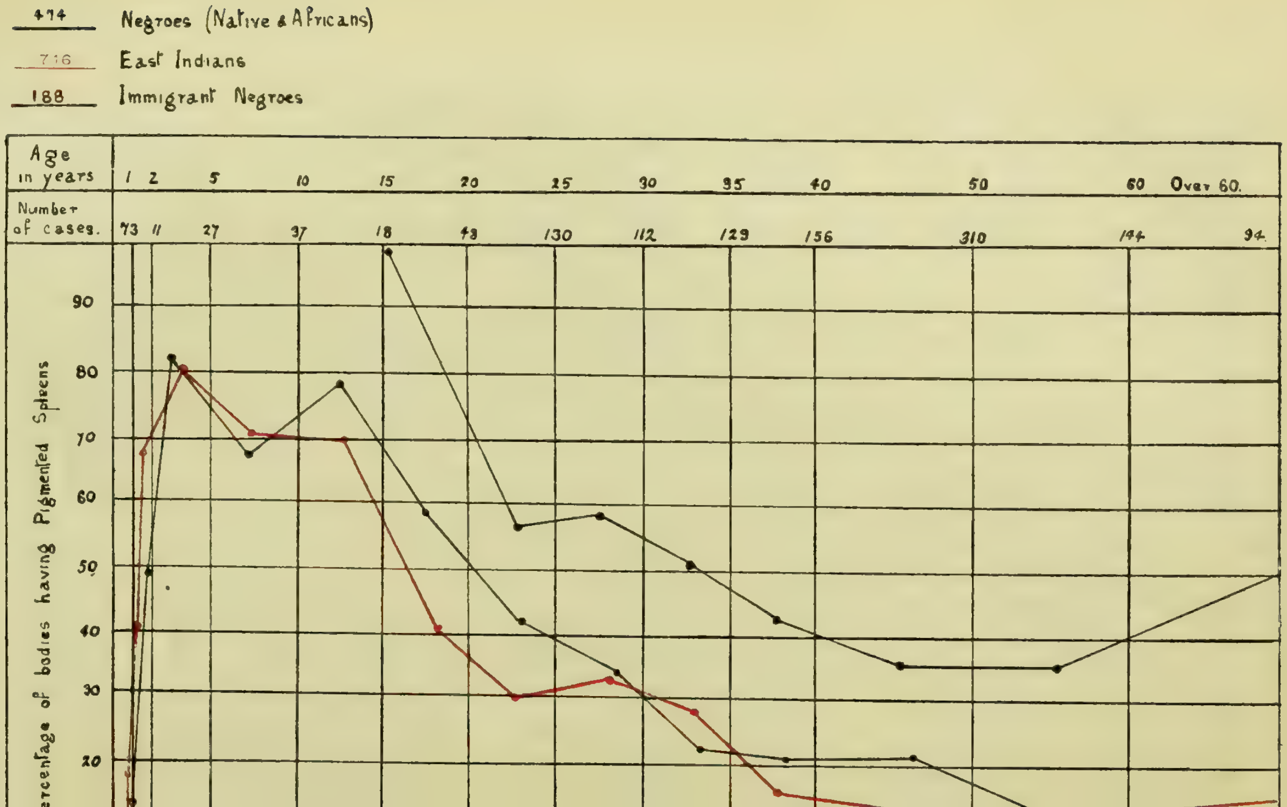
The error tends to be larger when the percentage of objects belonging to the particular class is near 50% of the total number of objects examined, than when it is a low or high percentage. We must therefore examine, for example, more leucocytes to obtain the percentage of mononuclears than to obtain that of eosinophiles; and more people if the parasite

or spleen index is near 50% than if they are very small or very large.

(9). *Variation of the malaria index according to age.*—It is now generally accepted that amongst the natives of a permanently malarious locality the signs of infection—parasites, fever, enlarged spleen—are generally much more frequently found in children than in adults. From this we infer that the individual can acquire natural immunity against the disease.

So far as I can ascertain, this fundamental law was first discovered and stated by C. W. Daniels as the result of his study of a large number of autopsies in British Guiana [1895]. He found that the frequency, not only of simple enlargement of the spleen, but also of the malarial pigmentation of the organ, varied from age to age. Both enlargement and pigmentation were absent in the bodies of infants less than one month of age; became more and more frequent in bodies of children up to five years of age, in about 80% of whom they occurred; and then became progressively less frequent with advancing age. These results were obtained from 1,289 autopsies on natives of the colony or of other malarious places — mostly negroes and East Indians. But different figures were recorded from 205 autopsies on persons, chiefly negroes, who were born in non-malarious places, and had come to the colony in early adult life. In nearly all of these the symptoms occurred shortly after arrival, and then fell in frequency with age, but never to so low a figure as in the case of the natives. The following are the curves for the negroes and East Indians. (See page 236).

Daniels remarked, "It is, I think, clear from these figures that malaria is, amongst those exposed (to it) from childhood, a disease of early life. . . . In adult life, amongst them, it is rare; whilst amongst persons of the same race, but not exposed in childhood, it is common in adult life. This, it seems to me, can point to only one conclusion, viz.: that immunity is acquired by a class (that is, natives) in the course


474 Negroes (Native & Africans)
716 East Indians
188 Immigrant Negroes
Age in years
1 2 5 10 15 20 25 30 35 40 50 60 Over 60.
Number of cases.
73 11 27 37 18 48 130 112 129 156 310 144 94.
90
80
70
60
50
40
30
20
Percentage of bodies having Pigmented Spleens

of exposure to the infection in this district for some twenty years; and, from the evidence of its frequency (evidence persisting for variable periods, in some cases for years), that this immunity is probably acquired by previous attacks." Daniels also observed that the spleen is frequently found to be enlarged, and very much enlarged, without showing any pigmentation at all, and inferred from this that there might be some other cause besides malaria for the enlargement.

In my study of kala-azar [1899] I came to the erroneous conclusion that it was nothing but severe chronic malaria. Arguing from Daniels's observations, I thought that the unpigmented enlargement of the spleen in this disease was due to numerous previous attacks of malaria followed by partial immunity and natural elimination of the pigment (plasmodin). Kala-azar is now known, of course, to be caused by another parasite; but I still think that the pigment may be eliminated in cases of malaria after the parasites have died out—thus explaining its absence from old malarial enlargements.

A little later, R. Koch observed that the parasites can usually be found more frequently in children than in adults, and suggested the examination of the blood for measuring malaria. His figures are given principally in his third Report [April 1900]. At Stephansort, out of 734 persons the parasites were found in 157, or 21·4%. In Kaiser-Wilhelm-Land, at Bogadjim, out of 22 children of under five years, the parasites were found in 13, or 59%; while none were found in 86 persons of over five years in age. At Bongu, out of 19 children under five years, the parasites were found in 12, or 63%; out of 17 children from five to ten years, in 4, or 24%; and out of 39 persons from ten to forty-five years, in none (note, large error of random sampling).

Almost at the same time, Christophers and Stephens published similar figures regarding studies in the Gold Coast [June 1900]. They state that in four places the parasite index found by them was 90, 75, 71 and 23% among "babies"; 57, 50,

75, 20% among children up to eight years; 28, (?), 30, (?)% in children up to twelve years; while children over twelve years showed parasites very rarely. They give no actuals.

During the same year, Annett, Dutton and Elliott made a survey of the parasite index in ten places in Nigeria [1902]. The collected results were as follows:—

Ages	0-1	1-2	2-3	3-4	4-5	5-6	6-7	7-8
No. examined	37	46	49	47	41	23	30	25
No. infected	10	29	31	24	20	8	2	11
Percentage	27	63	63	51	49	35	7	27
Ages	8-9	9-10	10+		0-5	5-10	10+	TOTAL
No. examined	16	14	40		220	108	40	363
No. infected	4	2	4		114	27	4	145
Percentage	25	14	10		52	25	10	40

The figures agree well with those of pigmented spleen rates given by Daniels. There is a curious drop at the 6-7 age. The authors' tables are well compiled; but the numbers for the separate localities are too small to furnish definite results.

Panse [1902] gives the following figures for Tonga, East Africa:—

Ages	0-½	½-1	1	2	3	4-5	6-7	Half-grown.	Adults	TOTAL
No. examined	16	9	25	20	28	48	84	314	1,683	2,227
No. infected	6	6	22	17	25	34	52	125	258	545
Percentage	37	67	88	85	89	71	62	39	15	25

Craig gives for Camp Stotsenburg, Philippines [1909]:—

Ages	1-5	5-10	10-15	Adults	TOTAL
No. examined	40	54	53	45	192
No. infected	30	20	13	28	91
Percentage	72	37	24	62	47

Similar figures are furnished by various authors, but the number of children examined is generally too small to yield reliable results. Often only percentages, which are, of course, almost useless, are given.

The comparative frequency of enlarged spleen in children has been known for a long time—certainly in India; but much more exact work on the subject remains to be done.

Craig well remarks [1909] that there are certain "rules

governing immunity." In heavily infected localities, where nearly all the natives are probably infected and re-infected in childhood, nearly all the survivors must be partially immune in adult age. On the other hand, where only a smaller proportion of the natives become infected in childhood, many of the remainder may be attacked for the first time at later ages. This must be especially the case where there are seasons, such as a sharp winter or a hot dry season, during which infection ceases. I notice, for instance, that the law of Daniels is not much insisted upon by Italian workers.

From these considerations an important deduction, exactly opposite to the common opinion, may be drawn. Where there is much malaria, so many of the native adults will be immune that few of them should show signs of infection ; and, conversely, where the adults are frequently attacked, the amount of malaria should be comparatively slight. In other words, if the malaria index *amongst adults* (parasites, fever, splenomegaly) is high, the malaria rate of the locality is probably low ; and where the adult *index* is low, the *rate* is probably either high or low. If, therefore, a considerable number of adults are coming to hospital with fever or enlarged spleen, we should believe, not that the malaria rate of the population is usually high, but that it is usually low—comparatively, of course. If malarial immunity were complete, and if every one were infected in childhood, no one would be ill afterwards. For example, if many soldiers in a regiment are attacked, we may infer that they have not been recruited from very malarious districts.

Unfortunately, however, we are not sure (*a*) how far immunity derived from a single infection protects against subsequent infections, and (*b*) how far immunity against one species of parasite protects against other species. Thus many partially-immune adults may suffer from short attacks of fever due to re-infection, especially by parasites of a species different to the one to which they have become habituated.

Practically, therefore, for the purpose of the measurement of

malaria, it is best to consider only the malaria index among children. Among adults the malaria index cannot yield very definite results: if high, it indicates that the malaria rate is comparatively low; if low, that the malaria rate is either very high or very low.

The study of large numbers of children is required to establish the age at which the malaria index is *highest.* Most of the figures suggest that that age is between one and three years. Similar studies are required to determine the age of maximum average spleen, and of maximum enlarged spleen.

(10). *Determination of the inoculation rate.*—According to section 26, the malaria rate of a locality is the percentage of the people who, at the moment of enquiry, contain plasmodia. But as many of these people must have been inoculated weeks, months or years previously, the malaria rate does not directly inform us as to what is going on at that moment—it instructs us regarding the past rather than the present. We may wish to know how many people are actually being inoculated during the week, month or year of observation; and if we can ascertain this we can obtain a measure of the actual *malariousness* of the locality at the moment when we are investigating the malaria in it.

This could be ascertained experimentally if we could distribute throughout the population a large number of healthy, non-immune immigrants, and could then observe accurately how many of them become infected within a given period: or if we could observe a large number of non-immune immigrants, and learn how long, on the average, they remain healthy. Such studies can sometimes be made with adults; but, unfortunately, Nature herself is always carrying out the experiment for us by providing a constant influx of non-immune babies, and we have only to ascertain the proportion of them which have become infected within a week, month or year after birth.

In his original article [1895], Daniels stated that in 43 infants who had died under one month after birth pigmentation

of the spleen could not be found; that it occurred in 12·5% of 16 infants of one to six months of age, in 11% of 14 infants of six to twelve months, and in 50% of 11 infants of one to two years of age. From these figures we gather that in British Guiana at that time the chances were small that infection should occur within one month, and increased by about 2% roughly for every month after birth—so that the chances were that about 50% of all newcomers would become infected in two years. Meanwhile, however, immunity would tend to reduce the cases. Of course, with such small actuals, the error of random sampling is very large.

R. Koch in his third Report [1900] states that in nine localities in Java, the parasites were found in 67, or 11%, out of 619 infants of under one year examined; and in 47, or 8·1%, out of 574 children of over one year of age.

The *infantile malaria index* can easily be obtained by the parasite test or spleen test, or both combined, and, with large actuals, will give a good direct measure of the inoculation rate. But it will be still more useful if compared with rates calculated according to sections 27 and 28.

As before, let p be the total population, and m and m_1 the proportion of infected persons at the beginning and the end of the enquiry respectively. Then at the beginning of the enquiry there were mp infected persons and $(1-m)p$ healthy ones. Now let rmp be the number of infected persons who recover during the enquiry—suppose that $r=1/5$ and that the enquiry lasts one month; and let $I(1-m)p$ be the number of healthy persons who become infected during the enquiry. Then by section 28, equation 4,

$$m_1 p = mp + I(1-m)p - rmp$$

or

$$I(1-m)p = m_1 p - mp + rmp$$

Thus suppose that, in a village of 1,000 people, there were 250 infected persons at the beginning of the month and 300 infected persons at the end of it. Then, obviously, though 50 infected persons recovered during the month, there was

nevertheless an increase of 50 infected persons at the end of it; so that 100 healthy persons must have become infected during the month. That is, $I(1-m)p = 100$.

Here $I = (m_1 - m + rm)/(1-m)$, and is the *inoculation ratio*; that is, it gives the proportion of people, healthy or unhealthy, inoculated during the enquiry. In the above example $I = 10/75 = 0{\cdot}133$. Out of the 750 originally healthy people in the village 100 were newly infected, and out of the 250 previously infected people 33 were *reinfected* (Imp gives the number of reinfections); so that altogether 133 persons out of 1,000 were inoculated during the month. The inoculation *rate* of the village was $I \times 100 = 13{\cdot}3\%$; that is, the chances of a new-comer in the village, or of a new-born child, becoming infected during the month was as 13·3 to 100.

From equation 1 of section 27, we might infer roughly that there were about 106 Anophelines to each person in this village. In fact, on comparing that and the following section, we see that $I = b^2saim$.

The fractions m, m_1, I and r are connected by the equation $m_1 = m + I(1-m) - rm$; and if we can ascertain any three of them we can calculate the fourth—which enables us to compare observed and calculated estimates. If the malaria is static, so that $m_1 = m$, then $I(1-m) = rm$. We may now estimate I from the infantile malaria index, m from the general malaria index, and r from statistics, and check our results by comparison. Suppose, for example, that $r = 1/5$ a month, and that 4% of the infants are infected in a month; then the general infection rate of the locality should be about 16·6%. Comparing these figures with observed estimates, we shall probably arrive nearer to the truth than if we had relied on calculation or observation only.

(11). *Practical conclusions.*—Having considered the various methods of measurement which may theoretically be adopted, it remains for us to select the best ones for practical use.

A. First suppose that we wish to form an *Actual Estimate*

of the *Malaria Rate*; that is, of the percentage of people who contain plasmodia at the moment of observation. We may employ (*a*) the parasite index, (*b*) the spleen index, and (*c*) the combined methods.

(*a*) The *parasite index* gives exact information only as to the lowest proportion of infected persons. The proportion of those who contain plasmodia in numbers too few to be found during the time allotted for the test may be very considerable, and has not been correctly estimated. The test requires a skilled observer, and the expenditure of so much time that it can be applied only to comparatively few people. The error of random sampling is therefore generally very large.

(*b*) The *spleen index* (by palpation only) can be quickly ascertained, by almost any intelligent person, for large numbers of people — so that the error of random sampling may often be reduced nearly or really to zero. A certain proportion of infected persons have no *palpable* enlargement of the spleen, and possibly in others with palpable enlargement (especially with great enlargement) the parasites may have died out in consequence of immunity; but by the penultimate paragraph of (2), these two sources of error (amounting to say 20% each) will tend to annul each other, so that the *spleen index should be a nearly correct measure of the true malaria rate.* Quinine, local conditions and other diseases (especially kala-azar) may affect the spleen index, but in most countries probably only to a small extent. *Percussion* of those who show no palpable enlargement will reveal a greater proportion of cases; but with this test the infected non-spleen ratio will not be balanced by the non-infected spleen ratio—so that percussion may give a ratio further

from the truth than that given by palpation alone. Percussion will also demand more time and skill.

(*c*) The *combined methods* (on the same persons) will yield a greater malaria index than each method by itself—often a much greater one than that given by the parasite index by itself, and a considerably greater one than that given by the spleen index. If, however, Laveran, Thayer, Mannaberg and others are right in their opinion that some enlargement of the spleen can be detected by percussion in almost all infected persons, then the combined parasite index and spleen index (by palpation alone) can yield little more information than that given by a careful spleen rate ascertained by percussion. The combined method will, however, require much more time, and will give no exact information regarding the non-infected spleen ratio.

Subject to correction, then, I conclude that, as a general rule, the *spleen index* (obtained by palpation only) is by far the best method of measurement. It requires no great skill; it can often be applied to almost all the people in a place; and it should yield by itself a very correct measure of the actual malaria rate. As a rule, local conditions, race and other diseases are likely to cause a percentage of error far smaller than that due to the insufficient random sampling which must almost always attend the laborious estimation of the *parasite index* (the advocates of which frequently overlook this important point). But of course, in the presence of other widespread causes of splenomegaly (kala-azar or some unknown cause), the spleen index by itself will not be so reliable.

Practically, therefore, it comes to this, that we must avoid two principal sources of error, (*a*) the possibility that the local splenomegaly may be due to other causes than malaria, and (*b*) the great error of insufficient random sampling. For the former purpose we must first satisfy ourselves, by the study

of hospital records (especially of autopsies), by consultation with local physicians, and by the blood examination of selected cases, that the local splenomegaly is really due to malaria. For the second purpose, if the first point is established, we should rely upon a large spleen census. The time required for a laborious study of the parasite index will generally be much better spent in extending the spleen census so as to avoid large error of sampling; and the combined method will only occasionally be required for some special purpose, such as ascertaining the infected non-spleen ratio, or the infantile malaria index.

We must, of course, always consider the season, the time allotted for the work, the amount of assistance available, and the population and area to be examined. For large populations the parasite index becomes almost impossible. Not the least advantage of the spleen index is that it can be applied equally well to a population of any size.

B. *The actual infantile malaria rate.*—This is especially valuable by (10) for determining the *inoculation rate.* It can often be obtained among infants on estates and factories, in soldiers' families, and in many villages. As, however, infants up to one or two years of age are not always very numerous among small populations, and the information required is important, it is generally advisable and possible to use combined methods for examining them. The ages should be obtained as accurately as possible for each month for two years, and the infants examined as regards spleen, parasites, fever, anaemia and history of illness.

The information obtained should be compared with the formula at the end of (10). If we assume, as argued above, that the general spleen index practically coincides with the general malaria rate, and denote the spleen ratio by S and the infantile ratio by I, then the formula becomes

$$S_1 = S + I(1 - S) - rS$$

If $S_1 = S$, $r = 1/5$ a month, and I is a monthly infantile rate, then we should have $I = S/5(1 - S)$.

It is, of course, best to examine, if possible, all the infants and to compare their numbers at different ages with the numbers given by, or calculated from, the last census of the population.

C. *Comparative estimates* from time to time or place to place.—For these we adopt the same methods according to the same principles. But we can now often utilise the valuable information given by the attendances at hospitals and dispensaries (5). That is, we obtain the ratio of attendances for malaria to attendances for all causes, and compare them from time to time or place to place.

Remember that, for comparative estimates, ratios of *indices* should, *ceteris paribus*, be equivalent to ratios of actual *rates*—which makes the task much easier.

The conduct of the malaria census will be referred to in connection with the whole campaign (section 40).

32. The Mortality and Cost of Malaria.—A little consideration will convince the reader of the difficulty of this part of the subject. The term *malarial death-rate* should mean the percentage of persons who die of malaria. Such rates can easily be obtained in the case of plague, cholera and other diseases in which the cause of death can be generally assigned with certainty ; but malaria is a benign and protracted disease which is often complicated and terminated by other maladies, such as pneumonia, infantile diarrhoea, dysentery, ankylostomiasis, and so on ; and it is often, perhaps usually, impossible to say whether death has not been due as much to one of these complications as to the original infection. Even with troops and prisoners the cause of death in such cases is often ascribed to one or the other cause on the evidence of the most predominant symptoms at the end. Even if we admit the correctness of the diagnosis as regards

deaths in hospital, still the hospital case mortality is no correct guide to the total case mortality, because only the more acute cases among the general population come to hospital at all. Probably the great majority of malaria deaths occur among the children of the poor, and in the tropics these are seldom brought to hospital, or even attended by medical men. The fact that in most tropical countries the cause of death is not necessarily certified by medical men adds still more to our difficulties. Indeed the so-called malaria death-rates given in many statistics are not even worth looking at.

The parasites of malaria cause death in two ways: (*a*) by direct action, and (*b*) by enfeebling the constitution.

Deaths by direct action probably occur only in "pernicious attacks." Celli (section 20 (6)) records that 330 pernicious attacks occurred in 33,507 cases — about 1%, and of these probably only a fraction were fatal.

W. H. Deaderick collects many useful facts in his text-book [1909], and gives records of case mortality amongst them. Out of 5,109,001 cases collected from twenty-five writers, 148,055 or 2·89% died (I judge that these were mostly severe cases treated in hospital). Out of 27,039 cases of *pernicious* malaria, recorded by thirty observers, 7,205, or 26·6% died. Out of 6,037 cases of blackwater fever (one of the most pernicious forms), 1,268, or 21% died.

In India for 1907 we have the following figures (combining intermittent and remittent fevers):—

	Strength.	Admissions.	Deaths.	Malarial mortality%.	Case mortality%.
European troops	69,332	10,662	14	·020	·13
Native troops	126,392	28,432	84	·066	·30
Prisoners	93,264	17,841	88	·094	·51

The highest malarial mortality here given, that of the prisoners, may be put at about 1/1000; but we can see at once that this is sure to be too low an estimate for a large general population. Troops and prisoners live in cleanly surroundings, are well housed, well fed, well treated in hospital,

and (still more important) are all adults. Moreover, in my experience, medical men rarely attribute death to malaria alone unless there are no very marked complications. The rate amongst a large, poor, general population, including children, is therefore likely to be several times greater. But we have no figures to indicate the difference.

As just mentioned, the deaths attributed in public statistics to malaria are generally worthless in the tropics, as they are not even certified to by medical men. They are often much too high, because the perplexed registrar has no other cause to give when dysentery, cholera, fits or debility fail him. For example, in Mauritius the deaths attributed to malaria constituted 14 *per mille*, or about half of the total mortality. In 1899, however, the medical certification of deaths was enforced in two districts of the island—with the result that the mortality ascribed to malaria fell in them at once from 45% of the total deaths to only 25%. From this we calculate that quite 20% of the general mortality was wrongly ascribed to malaria. But even with this correction many sources of error remain.

Is there no better criterion of the true malarial death-rate? In most civilised countries to-day the total death-rate is correctly reported: we may therefore obtain useful information by comparing the total mortality in various areas which differ in possessing or not possessing malaria, but which are alike in other respects. Take, for example, Mauritius (malarious) and the neighbouring Seychelles Islands (not malarious).

		1904	1905	1906	Means
Mauritius	Population	387,395	386,128	383,206	385,576
	Death-rates	32·2	40·6	40·0	37·6
Seychelles	Population	20,418	20,767	20,976	20,720
	Death-rates	16·1	15·0	16·5	15·9
Difference		16·1	25·6	23·5	21·7

For the seven years 1897 to 1903 the mean death-rate in the Seychelles was only 17·1 *per mille* against 34·7 in Mauritius. If the whole of this great difference is to be ascribed to malaria

alone, we must suppose that the true malarial death-rate in Mauritius may average something like 20 *per mille per annum* or more — that is, a mortality exceeding the total mortality from all causes in the Seychelles and in most British towns.

The figures are accurate, the deaths being carefully registered both in the Seychelles and in Mauritius, and the islands have a very similar climate. But in the mountainous Seychelles there are few or no Anophelines as in Mauritius. The population of the former, however, is of negro origin, and in the latter of Indian origin and much more dense.

The following are some figures for ten other neighbouring islands of the Indian Ocean—all not malarious. The populations were those of the census taken in 1901, and the death-rates were the averages for the following five years. *Rodrigues*, 3,437, 19·3; *Diego Garcia*, 526, 27·0; *Agalega*, 327, 31·2; *Peros Banhos*, 184, 24·0; *Coetivy*, 143, 36·4; *Salomon*, 119, 32·0; *Six Islands*, 117, 29·0; *St Brandon*, 87, 55·3; *St Jean de Nové*, 75, 16·0; *Eagle Island*, 74, 43·0. Total population, 5,134. Average death-rate, 31·5. Owing to the smallness of the population in the lesser islands the statistical error is large; but the mortality in some of them, in spite of the absence of malaria, appears to be as great as that of Mauritius. There is, however, an important difference. In Mauritius, the Seychelles and Rodrigues good medical attendance is available, but this, I understand, is not the case in the other islands, where syphilis, beri-beri, infantile tetanus, bowel complaints, ovarian and uterine diseases abound, and deaths in child-birth are common. Excluding these islands, therefore, the difference as regards the mortality between Mauritius with malaria, and Rodrigues and the Seychelles without it, remains a striking one. The large malarious island of Réunion (negroid population) has a mortality similar to that of Mauritius.

The entry of malaria into Mauritius in 1866 is interesting in this connection (section 30 (21)). All deaths in the island have been registered since 1831. In the thirty-six years from

1831 to 1866, the average death-rate was 35·3 *per mille* in a population varying from 93,000 in 1831 to 365,000 in 1866. In the next five years the death-rates were 120·5, 56·7, 35·0, 22·6 and 44·1 respectively; and in the forty years from 1867 to 1906 they averaged 35·7—almost the same as before the entry of malaria. Thus, apparently, while the disease caused a great increase of mortality for two years after its entry, it has caused no increase whatever since then! In my report [1908], however, I have discussed this anomaly. As Dr Meldrum suggested, the extraordinary low mortality of 1870 was possibly due to the death from malaria of many persons in 1867 and 1868 who would otherwise have died in 1870. The high death-rate before the entry of malaria was probably due to many epidemics of measles, small-pox, relapsing fever and cholera, and to the immaturity of medical and sanitary practice in those days. Since the entry of malaria there have been few of such epidemics, and medical and sanitary practice have greatly improved, while vaccination has been rendered compulsory—so that the death-rate should have fallen to the low level found in Rodrigues and the Seychelles. Hence we may perhaps assume that it has *not fallen* because of malaria. In other words, the disease has counterbalanced all the successes of medical science against other maladies.

The late Dr Meldrum, Director of the Meteorological Observatory in Mauritius, and an experienced statistician, studying the monthly death-rates from all causes in Mauritius before and after the entry of malaria, observed that they differed in an important particular. Before the malaria entered they remained roughly the same from month to month; after it entered the curve rose markedly during the malarious months. In the Seychelles and Rodrigues the monthly rates still remain uniform as in Mauritius before malaria entered. The following table gives the population and number of deaths.

	Average population.	January.	February.	March.	April.	May.	June.
Seychelles.							
1901-04	19,442	31	28	27	30	26	33
Mauritius.							
1861-66	345,275	1,003	936	987	920	972	898
1870-89	353,958	790	809	1,005	1,043	1,118	1,059
1904-06	376,974	1,012	981	1,307	1,314	1,357	1,262
	Average population.	July.	August.	Sept.	Oct.	Nov.	Dec.
Seychelles.							
1901-04	19,442	30	23	26	27	26	25
Mauritius.							
1861-66	345,275	970	971	947	970	910	967
1870-89	353,958	1,004	893	783	759	718	740
1904-06	376,974	1,597	1,284	1,125	1,089	1,285	909

In the first two lines the monthly deaths are fairly uniform; in the latter two they are lowest in November or December, before the malaria season, and highest after that season. Meldrum also enunciated another law based on the careful study of Mauritian statistics, namely, that the highest death-rate generally occurs about two months after the heaviest rainfall. But the figures for other countries show that there may be many disturbing factors—heat, cold and epidemics. Nevertheless, Meldrum's laws should be used, together with other data, in the study of local malaria death-rates [1881, 1890]. We should endeavour to ascertain the average ordinary mortality, exclusive of epidemics, during the non-malarious months, especially those just before the commencement of the increase probably due to malaria. The difference between this average and the total mortality during what we have reason to suppose are the months of malarial mortality should give some rough indication of the amount of the latter. S. R. Christophers has recently suggested [1910] that rises in the total mortality, *minus* other epidemics, should indicate epidemics of malaria. They should also indicate endemic variations. But of course in all such studies an accurate knowledge of local circumstances—climate, rainfall, other diseases—is required.

So far as I can see the only method by which we can hope

to ascertain the increase in the general mortality really due to malaria would be to undertake a careful comparison of the death-rates and spleen rates over a large area and for a considerable period. The task would be a suitable one for all Health Departments, and ought to have been performed long ago — if only to determine that fundamental question, the actual amount of mischief caused by the disease.

In 1898 I proposed to undertake such a survey in Mauritius, and collected a number of figures from the sugar estates [1898]. Unfortunately, the death-rates could not be obtained for the period during which the spleen rates were taken; and other fallacies invalidated the data. In section 22, however, the rates are given for the various districts of the island. The healthiest district, Moka, showed a spleen rate of 4·4% and a death-rate of only 18·7 *per mille*, while Black River had a spleen rate of 56·7% and a death-rate of 39·5% (on the estates). This again suggests—as we saw from a comparison of Mauritius and the Seychelles — that malaria, when severe, may actually double the general mortality. But before accepting such an appalling estimate I should like to see the subject worked out step by step — I should like to know for several countries and races the rise in the mortality step by step for every rise of 10% in the spleen rate. Until this is done we remain merely in the clouds of medical conjecture.

What precisely is the economical loss to the community caused by malaria?—From the preventive point of view this is perhaps the most important question before us; because, obviously, it governs the question of the expenditure which may be demanded for the anti-malarial campaign. In order to answer it we must know (*a*) the real malarial mortality; (*b*) the constantly sick rate (section 31 (6)); and (*c*) the local values of human life and labour at various ages and in various social classes. Considering the importance of the theme, I had hoped to give here a detailed estimate; but I find that the data are quite insufficient. We do not know even the real death-rate,

much less the death-rates at various ages; and the values of life and labour vary so much that different estimates must be prepared for each country.

Many such estimates have been attempted. I will mention here as an example only one by Dr Bolton, Medical Officer of the Immigration Department of Mauritius (because I have already given so many statistics of that colony). There are about 39,000 adult male coolies on the sugar estates, of whom about 15% are incapacitated by malaria for three months in the year. That is, more than 500,000 days' work is lost annually. The day's work is worth 1/4 rupee to coolies and 5/4 rupee to the planter; so that the coolies lose about 125,000 rupees and the planters about 625,000 rupees per annum, or 750,000 rupees altogether. Besides the males there are about 8,000 women working on the estates, and, at the same rate, these lose 108,000 days' work, worth 21,000 rupees at 0·20 rupees a day. Similarly, outside the estates, there are 30,000 Indian coolies, earning 0·5 rupees a day; and these lose about 20,250 rupees a year. Thus the workers lose about 166,250 rupees *per annum*, and the employers considerably over 650,000 rupees. With hospital and other expenditure Dr Bolton estimates that malaria costs Mauritius, with its population of 383,000, about 1,000,000 rupees a year (a rupee is about 1/15 pound English)—that is, about 2·6 rupees *per* head. Enhancement of wages, cost of invaliding, and loss of market are not considered. Similarly, L. O. Howard estimates that malaria costs the United States $100,000,000 a year (section 41). See also section 65 (9).

CHAPTER VI

PREVENTION

33. List of the Possible Preventive Measures. — One advantage of the malaria formula given in section 28, equation 4, is that it enables us not only to name the various preventive measures, but also to obtain some quantitative estimate of their relative utility. Suppose that an anti-malaria campaign is contemplated in any locality, and let m be the proportion of infected persons at the beginning of the campaign, and m_1 be the proportion at the end of a given period. Then by the formula

$$m_2 = m_1 + b^2 sai\,(1 - m_1)\, m_1 - rm_1.$$

Hence m_2 will be less than m_1, that is, the malaria will be reduced, if we can make suitable modifications in the factors b, s, a, i, r. Such modifications can be made in various ways, as follows:—

The *biting factor*, b, represents the average proportion of Anophelines which succeed in biting individuals. We have roughly estimated it at 1/4. Obviously, if we diminish this fraction we shall diminish the malaria in the locality. This can be done in many ways. The use of ordinary *mosquito-nets*, of *wire-gauze* to the windows and doors of houses, of *punkas and fans* (which drive away the insects), of culicifuges, that is, substances which, when applied to the skin, tend to keep them off, and even the use of *smoke*, *woollen clothing* and *bed coverings*, must all tend to have the desired effect. But, of course, if we hope to reduce the malaria throughout a considerable

population, such measures must be adopted by a sufficiently large number of people. No appreciable effect will be produced on the general malaria rate if only a few persons take the trouble to use them, because b expresses the *average* proportion of insects which succeed in biting.

The next factor in the equation is s, which denotes the proportion of insects that succeed in maturing the parasites, and which we have generally taken at about 1/3. This factor depends principally upon the species factor of the local Anophelines (section 30 (11)). Certain species certainly succeed in maturing the parasites more easily than others do, and, moreover, it has been suggested that certain foods inhibit the growth of the organisms in the insects, while cold also does the same thing. I fear that we are unable to modify this factor by itself.

The next factor, a, denotes the number of different Anophelines to each person, and can be modified by any of the measures adopted for *mosquito reduction*; that is, the *adult* insects may be killed by hand, or by various culicicides, or perhaps by the introduction of enemies, such as certain bats or birds. Or the larvae may be destroyed in numbers of ways, such as emptying out the water containing them or applying larvicides of various kinds, or introducing certain fish or beetles. Or the breeding of the insects may be checked by removing water suitable for their larvae or the weeds in which they often flourish. Or the locality may be rendered unsuitable for a given species of Anopheline by doing anything to modify the conditions which that species loves, such as removal of forest or undergrowth. Lastly, reduction of the biting factor, b, may tend to reduce the total mosquito density, according to section 29 (7).

The factor i denotes the *average* proportion of infected persons whose blood contains the sexual forms. This can be modified to a certain extent by the careful treatment of the patients—though we must remember that quinine does not

have much influence on the crescents when these are once produced (sections 23 and 65).

The factor r denotes the *average* proportion of infected persons who recover during a given period, and this can certainly be very largely modified by suitable treatment of the sick. It is true that certain authors declare that quinine does not have much effect on relapses (section 23), but in the opinion of a majority of observers early and continued medication certainly produces marked results. Such treatment of persons who have had declared symptoms of the disease should be called *case reduction*; but we should observe that a similar treatment may be commenced in persons before they have had any symptoms at all. That is to say, all persons in a locality may take quinine continuously on the chance that they may have happened to have become infected without their knowledge, and in the hope of checking the parasitic invasion at its very outset. This method of dealing with the disease should be called *quinine prophylaxis*. For case reduction we treat the sick, for quinine prophylaxis the healthy.

It will therefore be observed that we have many preventive measures against malaria; but the ones given do not close the list. Another important measure is that of *segregation*. Instead of destroying the local mosquitos or their breeding-places we may segregate ourselves or a number of people from their haunts; and, obviously, this will have the same effect upon the segregated people. Or we may segregate ourselves or a number of people from the infected area by removal to any locality where the original malaria ratio m is less.

The reader will now do well to suppose various values for the factors in the equation and then to calculate that of m_1 on these suppositions. Suppose, for instance, that the malaria ratio has been remaining at the static limit of one-half, so that by section 28, equation 4, the number of Anophelines, a, should be about 77 to each person. Now suppose that a is reduced to one-half: then m_2 falls from 1/2 to 9/20. Next, suppose that

b is reduced to one-half: then m_2 falls to 17/40—a slightly lower figure, because the factor b in the equation is squared. Lastly, suppose that r is increased by proper treatment to twice the previous recovery rate of 1/5 : then m_2 becomes 4/10—which is still lower. But if we increase the recovery rate we are likely also to decrease the proportion of persons with gametids in their blood, i. Suppose that if the first is doubled the latter will be halved : then m_2 falls from 1/2 to 7/20. Lastly, suppose that several of the factors are changed simultaneously — for example, reduce a and i to 1/2 and double r: then m_2 falls to 13/40.[1] This subject will be discussed again in section 39, but the reader should familiarise himself at once with the ideas involved in order to understand the following matter.

Many of the measures were first named and discussed in my little book called "Instructions for the Prevention of Malaria Fever," and my series of articles in the *British Medical Journal*, both published anonymously in the autumn of 1899. The principles then laid down by me have since then been repeated in many papers and text-books. We now proceed to examine them in detail.

34. Protection against the Bites of Mosquitos.—(1) *Portable bed-nets.*—The bed-net was well known to the ancients under the name of *conopeum* (section 1). It is now used almost everywhere in the tropics by those who can afford it, and adds greatly to the comfort of life, apart from the prevention of malaria. Long before the connection between malaria and mosquitos became known, General Outram of Indian fame, and Emin Pasha of African fame, and others, attributed their immunity from the disease to their care in the use of the net. In fact, when I went to India in 1881, it was a common saying amongst sportsmen, planters and many residents in that country, that the way to keep off malaria was to use a net under all circumstances.

[1] Or we can use the static formula, section 28, equation 7.

But the net must be used properly, and the following rules carefully attended to:—

(*a*) Not a single rent or hole in the net must be allowed. If there is one, mosquitos, which spend the whole night in exploring every inch of the net in the hope of reaching the sleeper, are sure to find it and enter. Not only should there be no rent or hole, but the net should be so hung that no aperture for entry is left. Thus it should always be hung inside the poles provided for the purpose, and tucked continuously all round under the mattress. It should not be hung outside the poles, because then it cannot be tucked in satisfactorily in the manner mentioned; and it should not be allowed to hang down to the floor unless heavily weighted, because then it is apt to be blown up by the wind, thus allowing insects to enter. Moreover, if it is hung down to the floor, insects which are hiding under the bed during the daytime will often be included. The servant should be instructed to let down the net before dark in the evening, and to see that no mosquitos are inside. If mosquitos do find entry, it is always due merely to carelessness.

(*b*) Do not have any opening for the purpose of entering the net. Such openings are often used in Europe and allow the insects to go to bed with the sleeper. When entering, one should lift the lower edge of the net from the mattress as little as possible and slip in with a twisting movement, so as to exclude stray mosquitos which may have been hovering round outside. If possible, use a large bed and a large net in order to avoid the hands, knees and elbows being pushed against the gauze during sleep, and thus being bitten through the net by mosquitos outside. If no large bed and net are available, this contingency

should be guarded against by sewing a loose valance of gauze round the lower part of the net, about 9 inches (23 cm.) above the upper surface of the bed, the valance being tucked under the mattress together with the net.

(*c*) The thread of the netting should not be too thick, as if it is so it will exclude air. The mesh should be neither too small nor too large, the general size fixed being about eighteen threads to the inch (seven to the cm.). No part of the net should be made of any closer texture, as this is quite unnecessary and excludes the air. For example, where, as in India, punkas swing over the bed all night, a top of calico merely excludes the breeze and adds to the discomfort of the sleeper. Of course, where sand-flies (*Simulidae*) are present, the whole net must be made of closer texture, such as muslin or cambric. These insects inflict worse tortures than mosquitos do, and have also been proved to carry a particular kind of fever. The netting should always be white to permit the easy detection of insects.

(*d*) In the great heat of the tropics, the manner in which the net is hung is very important. If it is arranged in thick or loose folds it will exclude the cooling breeze. The net should always be stretched as tight as possible in every direction, in order to allow the air to pass freely through it. In fact, those who complain that they cannot sleep under a net because it stifles them nearly always neglect this precaution. A tightly-stretched net with a thin thread excludes very little breeze. For this reason the pattern of net is important. In my opinion much the best nets are the square ones which are used almost throughout India, and which are capable of being stretched in the manner described. Nets which are bunched up

at the top and hung from a single rope are much more close and uncomfortable for the sleeper; but there is a good pattern which contains a circular hoop a yard or two above the bed, permitting the necessary tight stretching.

(*e*) Many patent patterns are advertised, and nearly all of them are good ones. Different patterns are required for fixed domiciles and for travelling. The traveller needs a portable pattern which can be set up very quickly in any locality where he may require to sleep. Bedsteads with attached nets are also on the market, and can generally be obtained locally. Special patterns have been suggested for soldiers and others who have to bivouac in the open air. I generally carry a small square netting which can be hung inside the berths on board ship, because shipowners are still barbarous enough not to exclude mosquitos by wire-gauze at the ports (section 46).

(2). *Portable mosquito-proof rooms.*—These are simply large nets, generally square and capable of holding the entire bed, together with a small table and chair, and perhaps a rotatory fan. Several special patterns are on the market. They are very useful for travellers. Entirely mosquito-proof tents have also been advertised, and should certainly be always used in warm climates. The one difficulty about such structures consists in the arrangements for entering; but in my opinion it is not advisable to provide a special door for doing so, as one can always creep in under the lower edge of the net.

(3). *Fixed mosquito-proof rooms.*—These are found in several colonies, and consist of a wire-gauze chamber supported by wooden uprights fixed in the floor. The mode of entry is by means of a self-closing door. The wire-gauze is in one respect preferable to the cotton netting, as it allows more breeze to enter, but on the other hand mosquitos which have entered cannot be so easily detected against the darker thread of the

wire. Of course, such chambers are constructed within the ordinary rooms of a house, or in the veranda. I have long advocated the use of them, during very hot weather, either in the open or on the roof of the house.

(4). *Mosquito proofing of windows and doors.*—This has been long in use in the Southern States of America, and has been much advocated by French and Italian writers on malaria. The net may be of cotton, but this, of course, soon rots away, so that wire-gauze is always better. I am very strongly in favour of this method of protection, which I have seen in use in Lagos, Panama and elsewhere. It adds greatly to the comfort of life in the tropics. It excludes not only mosquitos, but flies, moths and other insects, bats and birds. It also excludes the glare of the sun during the day, and the damp exhalation rising after heavy tropical showers. At the same time it allows the breeze to enter almost unimpeded.

The mesh of the net should be the same as that used for bed-nets. The wire may be either of "tinned iron" or of copper or brass. Roughly speaking, the price of the former is about twopence a square foot and of the latter about three-pence a square foot in British colonies. Some years ago a number of tests were carried out by the Crown Agents for the Colonies on behalf of the British Colonial Office, showing that tinned iron soon rots away in consequence of damp. But I once saw in Lagos a greenhouse covered in entirely with tinned iron wire-gauze which had remained quite sound for more than five years, even in that warm damp climate.[1]

The question how the gauze should best be applied to the windows has been much discussed, especially by Laveran [1907], who gives many diagrams and pictures. As a matter of fact, the question has generally to be decided according to the form of the windows already existing in a house. The problem is an easy one with sliding sashes. In such cases I advocate that the wire-gauze be permanently nailed, with nails

[1] See also sections 46 and 55.

of the same metal (in order to prevent galvanic rust), outside the window; that is, if the shutters are hung inside. For French windows, which open outwards on hinges, the problem s much more difficult, because if the wire-gauze is fixed inside, the windows cannot be opened. Many types are adopted in such cases, but they are not always satisfactory. Movable wire-gauze frames which require trouble to put up are sure to be neglected by servants. In my opinion it is best in such cases to refit the windows so that they can be made to open inwards, and then to fix the wire-gauze outside, or *vice versâ*. Further details are better left to the intelligence of the reader. In climates which are never cold, the glass of the windows can be replaced entirely by wire-gauze, thus simplifying the problem.

Doors afford another problem. The Italians have long advocated double swing doors with a protected entry, but this is necessary only where the insects abound very much. Automatic swing doors generally suffice. Of course, in order to protect a room or a house completely, every possible orifice, including key-holes, chimneys and chinks round doors and windows should be properly closed or guarded; but the details are so numerous that it is useless to give them in writing.

Wurtz [1907] states that a solution of commercial silicate of potassium in its own volume of water, if painted several times with a brush, without drying between the coats, upon a piece of ordinary cotton netting stretched on a window-frame or elsewhere, will make the netting much stronger and more resistent to water or fire. The painting can be done in five minutes. The solution dries in an hour. The mesh should be large, as the interstices are partially filled by the solution. In this manner cotton netting can be utilised for window and house protection. The suggestion is a very good one and might be developed further.

(5). *Mosquito proofing of verandas.*—This adds largely to the expense of protecting houses, but is, of course, a very great addition to comfort. A large part of the Meteorological

Observatory in Mauritius was proofed in this manner at the cost of several thousand rupees, and the measure is much adopted in Panama and elsewhere. A much less costly procedure is to protect a single corner of the veranda. This was insisted upon by Sir William MacGregor in Lagos, and is now adopted in that colony. Something of the kind should always be used, as people must have somewhere to sit in during the evening.

(6). *Protection for the hands and feet.*—In 1898 many writers advocated protection of this nature. It was suggested that whole armies should go on the march with veils over their faces and thick woollen gloves — a rather difficult suggestion for the tropics. The Italians have dealt with this matter at considerable length, and Laveran quotes and figures the protection given to Japanese soldiers in Formosa and Manchuria. Special boots to protect the ankles have also been recommended. Personally, I think that such means of protection are too uncomfortable for general use, and I much prefer a hand fan (mentioned presently).

(7). *Medicinal protectives.*—Many attempts have been made to find protective applications to the skin. Oil of lavender, of eucalyptus, petroleum, ammonia, powdered sandal-wood, and many patent fluids have been recommended to me for the purpose, but when I have recommended them to others, I have not always received favourable reports regarding them. Laveran [1907] mentions numerous statements on the subject regarding sulphur, petroleum, a mixture of tar and oil, and quassia. Vaseline (100 parts), naphthaline (10 parts), camphor (1 part) has been recommended; but he adds that his own experiences were unfavourable with such substances. Fermi and Lumbau [1900] tested many substances without marked success. Celli [1901] states that the old custom of hanging bags containing garlic or camphor round the neck originated in their protective effect against mosquitos.

We should observe that experiments on the point must

be very exact to be convincing; that the effect of volatile substances is sure to be transient; and that the use, even of successful protectives, can only be exceptional, as when mosquitos are very numerous, because few people will ever be persuaded to smear themselves constantly with them, and because they would probably affect the skin after a time. Such protectives do not come at present within the domain of practical sanitation.

(8). *Constant movement.*—As a general rule (but not always), mosquitos bite only when their victim remains quiet. Hence I have formed the habit, when in danger of being bitten, of maintaining constant small movements of the hands, feet and head. I find this, together with the use of a hand fan, far preferable to the employment of hot gloves, thick clothing or inunctions.

(9). *Fans and punkas.*—A palm leaf can be purchased almost anywhere in the tropics for a penny, and serves not only to drive away mosquitos, but also to keep the body cool.

Fans rotated by electricity or small hot air engines are now frequently used in the tropics by those who can afford them.

Punkas consist of a board or canvas stretched upon a wooden framework, hung by one edge from the roof, and swung backwards and forwards by means of a rope or pulley worked by hand labour or (in barracks and hospitals) by machines. The constant movement and the current of air prevent mosquitos biting even a sleeping person. Probably the alternating movement has a better effect for this purpose than the uniform movement of a rotatory fan. The punka should not consist merely of a pole with a fringe of cloth hung from it, because the yielding fringe gives no current of air.

(10). *Some results.*—Perhaps the first example of the utility of ordinary bed-nets was cited by myself [22nd July 1899]. In October 1898 a company of the "Boys' Brigade" of Calcutta went to live for one week in a house in Barrackpur Park, near

the River Hoogly. The three officers and a friend slept in bed-nets, but thirteen boys did not. The officers and their friend remained well; but all the boys, together with a sister of one of them, were attacked a few days later. The first boy attacked, died. Mosquitos were very numerous. Three native servants (without nets) also suffered.

In 1899 also many experiments were made on the Italian railways by screening stations and houses of employees. The results were summed up as follows by Celli [1902]. Out of 4,363 completely protected persons, 1·9% became infected and 21·1% had relapses. Out of 5,165 incompletely infected persons 3·3% became infected and 20% had relapses. Out of 802 persons protected only during sleep 10·9% became infected. The local fever index was 40-60%, or even more.

Laveran [1907] quotes an experiment among Japanese troops in Formosa, of whom, out of 646 unprotected men, there were 285 cases of malaria, and out of 115 protected men there were none; and also cites several similar small experiments in Corsica, Algeria, Holland and Senegal. In nearly all of these cases, however, quinine as well as protective screening was used. The experiment of Sambon and Low (section 7) served a useful purpose at the time in advertising the mosquito theory, but there was no very strong scientific evidence that the subjects would have been infected during the experimental period, even without the protection.

Further experiments should be performed in order to ascertain the effect of mechanical protective measures alone.

35. Mosquito-reduction.—This can be effected (*a*) by direct destruction of mosquitos or their larvae, and (*b*) by rendering houses or localities less suitable for them or for the breeding of them.

(1). *Destruction of adults by hand.* — Large numbers of mosquitos can be destroyed in houses by a very simple device —a small *hand-net.* This is made for a shilling or two by

tying a piece of flexible cane or other wood or wire in the form of a loop, to which a bag of white netting about 18 inches deep (4·5 decimetres) is attached. The net must be white and not green, in order to enable us to see the captured insect through the mesh. Any one can easily secure his persecutors by means of it, and thus obtain respite for some time.

Sir William MacGregor employed a boy to kill mosquitos in this manner in his house in Lagos in 1901. In lofty rooms a *long-handled net* may be required to catch the insects under the roof.

Colonel Gorgas and Mr Le Prince advocate (sections 42 and 43) the employment of men for this purpose and give examples. I think that every householder should possess such a net and should instruct his servants to use it regularly. The same thing should be done in hospitals, barracks, military camps and jails.

Much more could be accomplished by this simple method than has yet been attempted. Boys become very expert at catching mosquitos. The offer of rewards of a penny for fifty Anophelines may quite possibly lead to a large slaughter by village children. Probably, for unit of cost and trouble, more mosquitos can be killed by hand than by fumigation.

(2). *Fumigation.* — This method has long been employed, especially in Italy and the United States. The discovery of the carriage of yellow fever by *Stegomyia calopus* gave great stimulus to it for sanitary purposes. In yellow fever it is absolutely necessary that every *Stegomyia* which has bitten the patient should be killed for fear lest it may infect other persons; and this is rendered easier because the yellow fever patient remains infective only for three days, and because *S. calopus* is a domestic insect. But destruction of adults is not so useful for the prevention of malaria, because patients continue to be infective at irregular periods for a long time, and because the carriers are often wild insects (section 29 (4))

which may remain only an hour or two in houses. Hence the expensive and troublesome process of fumigation is not much employed in malaria prophylaxis.

Celli and Casagrandi made experiments with many substances [Celli, 1901]. Thick tobacco smoke and chloroform appear to be the best, and have been known for a long time. Sulphur dioxyde, hydrogen sulphide, coal gas, formaldehyde, kill in one to two minutes (but, curiously enough, acetylene gas seems to be quite innocuous). Fumes of pyrethrum powder causes apparent death in five minutes and actual death in eight hours. Numerous agents for the destruction of insects in greenhouses are on sale and are equally effective against mosquitos.

Boyce [1909] describes very thoroughly, and with good illustrations, the methods of fumigation practised against yellow fever in America. His recommendations have already been given (section 12).

In India fumigation is seldom used: and it would be difficult to apply in the thatched mud huts or leaf shelters of the poor. But I think that destruction by hand could be employed more than it is.

Mosquito-traps were also described in sections 12 and 60. Wells, cellars, privies, stables and dark rooms are natural traps in which the insects often congregate in large numbers and can easily be destroyed.

(3). *Introduction of natural enemies.*—Bats, birds, lizards, dragon flies and other flies are known to catch mosquitos; but whether their presence makes any marked effect on the number of the insects is another question, which, so far as I know, has not been studied.

It has been known for a long time that small fish devour the larvae; and indeed the larvae were used for feeding the fry of British trout introduced into India, in my memory, before 1890. Fish, however, vary greatly in their liking for the larvae. Thus in Mauritius the small gold fish which abound in ornamental waters eat them, but not voraciously. On the

other hand, I saw in India in 1890-1899 minnows which would devour a dozen or more in a few seconds and would visibly swell after the meal. The waters of Panama contain a minute fish, less than an inch long, which is equally fond of them, and which thrives in quickly running streamlets. The same is true of the common small fish at Ismailia. On the other hand, large fish often disdain such small prey. I saw a ditch in Calcutta full of small fish and also absolutely thick with mosquito larvae; and have observed fish and larvae living together in rice fields.

Boyce [1909] well describes the larvicidal propensities of *Girardinus poeciloides*, popularly called "millions;" a minute fish probably allied to the one which abounds at Panama. Mr C. Kendrick Gibbons in 1905 attributed to it the immunity of Barbados from malaria. The people keep these fish even in water receptacles, of which Boyce examined sixty without finding larvae. The Imperial Department of Agriculture in Barbados has introduced millions into Jamaica, St Kitts and Antigua. Further references to larvae-eating fish will be found in Chapter VIII.[1]

Tadpoles, *Notonecta* (the *water boatman*), and the larvae of several insects eat mosquito larvae; and indeed the latter are often cannibals.

This part of the subject is of great importance and is now receiving considerable attention. Obviously, the success of the measure must depend largely upon the habits of the local malaria carriers—whether they breed mostly in permanent pools where fish and their other enemies also can live, or whether they abound elsewhere, as in evanescent rain-water pools. The question depends entirely upon the local conditions and requires local study.

No cheaper and better anti-malaria measure than the

[1] J. Chaytor-White suggests the importation of "millions" into India [1910]. He reproaches me for not recommending the measure for Mauritius. This was due to oversight, and the fish are being introduced.

extermination of Anophelines by natural enemies could be conceived or devised—were such a thing possible. It may be possible in some places; while for other places we may not be able to find the suitable enemy. Unfortunately, experience proves that animals and plants often survive, even in large numbers, the introduction of their most deadly enemies; they succeed in finding some refuge or other, and a state of commensalism is finally arrived at. For example, Anophelines abound in the Panama Canal Zone in spite of the fish referred to. It is only the academical hygienist who believes that any measure which suggests itself is sure to be practically useful. It may often happen, too, that the expense of maintaining the "enemy" will be greater than that of removing the breeding water entirely. Nevertheless, we should seek what allies we can find.[1]

It has often been observed that parts of India where *M. rossii*, which seem not to carry plasmodia, abounds are comparatively free from malaria. Quite possibly it may tend to crowd out other species, thus reducing them below the malaria-bearing limit, although it does not exterminate them entirely. Experiments on this point are much required, and I have suggested them tentatively for Mauritius. The comparative freedom of large tracts where the conditions appear to be favourable must be due to some cause. If we can ascertain the factor we may be able to introduce it elsewhere (section 30 (10) and (11)).

H. Marcus Fernando, in a suggestive paper just to hand [1910], points out that though Colombo, Ceylon, is surrounded by low-lying grass fields, subject to periodic inundations, it is practically immune from malaria. This he attributes to swarms of minnows and other enemies of mosquitos. He states also that very hot dry seasons are often followed by much malaria, simply because they tend to destroy the aquatic enemies of the larvae. These views accord exactly

[1] See also section 54.

with those already expressed in section 30 (1), (6) and (10). So also rice fields are often not malarious—as in Madras town.

(4). *Destruction of larvae*—The methods employed depend on where the larvae are found.

For *small movable receptacles* of water, such as broken bottles and crockery, empty tins, flower-pots, gourds, old buckets and tubs, fire-buckets, fallen palm leaves, etc., emptying out suffices for the moment. But such articles ought to be collected in suitable dust-bins, and the laws regarding the proper deposition of rubbish ought to be strictly enforced.

Oiling.—This method, long known,[1] was first elaborated as a public health measure in my reports on Sierra Leone [1899]. It is employed wherever the previous methods are inapplicable, or where the water is required for use, or where more permanent measures cannot be adopted at the time—that is, for water butts, metal cisterns of drinking water, wells, garden cisterns, washing waters, cesspits and so on. The method has now become familiar to all. The oil is poured on the water and broken up so that it quickly forms a thin film all over the surface. The larvae and pupae are no longer able to keep the surface by surface-tension and quickly drown — Laveran thinks that the oil chokes the breathing tube [1907].

The oil may be poured on the water out of a tin or kettle, or by means of a special syringe. For small pools, I advocated that each man engaged on the work should be provided with a tin of oil, and a bunch of rag tied to the end of a stick. The rag is dipped in the oil, which is now "painted," on the surface of the pool—but such details are unimportant.

It is best to use the cheapest oils in the local market. Refined petroleum spreads rapidly and well, but evaporates quickly. Crude petroleum is cheaper and makes a much more lasting film. For Ismailia I recommended an equal mixture of the two. Celli [1901] said that 0·1 c.cm. was the minimum amount required per square metre of surface, but Fermi

[1] Howard [1893] and Nuttall [1899].

and Lumbau [1900] require 5·0 c.cm., and others various amounts. Experiments *in vitro* are of little use; and the amount required depends on the quality of the oil, the heat of the sun, the amount of vegetation, *débris*, etc., and should be determined locally. Logan Taylor and I found crude creosote to be very useful in Sierra Leone; it is more deadly, but also much more expensive than crude petroleum. For killing larvae in drinking waters, Strachan recommended me to use eucalyptus or juniper oil.

For pools it is advisable, and indeed sometimes necessary, to remove vegetation before applying the oil.

My brother, E. H. Ross, described an automatic oiler for cess-pits at Port Said, and Gorgas and Le Prince mention "cotton waste" soaked in oil.

Celli and Casagrandi [Celli 1901] and others describe experiments with many substances. Aniline dyes are active destroyers. Gallol and Green Malachite kill to a certainty in solutions of 0·0125/1000 and 0·25/1000, and a mixture called "larvicide" appears to be very effective. They diffuse themselves continuously in water, are not volatile, and do not poison men, cattle, or crops. Celli says that larvicide costs from 12 to 56 lire for destroying larvae in 10,000 cubic metres of water. It is said to have little action on the pupae.

Oiling should also be used for the tins of water placed under the feet of tables and meat-safes in order to exclude ants; and for saucers under water bottles, flower-pots and filters.

Small pools of water on the ground, especially those which form after heavy showers, or in rocks along the margin of streams, or in gutters, open drains, and stone water-channels, can often be quickly brushed out, water, larvae, and all, by means of a broom.

Holes in rocks and trees are best dealt with simply by filling up with earth or gravel as a temporary measure, or with concrete as a permanent one.

Plants which breed mosquitos should be removed if possible. In Mauritius, *Bilbergia splendida*, a wild pine-apple with a

gorgeous flower, was planted in most of the gardens and kept the houses supplied with innumerable *Scutomyia notoscripta* Skuse, until the wiser inhabitants rooted them up during and after my visit. Cut bamboo stems should be cut again flush with the next knot, or with the ground. Unfortunately, mosquitos often breed in palm trees and sugar cane — in which case I can recommend nothing but grease being applied at the off-shoot of the leaves. The householder has often to determine whether he will sacrifice his plants to his comfort. It is mostly the *Culicines* which breed in such situations.

Rain-water gutters along the eaves of houses are best dealt with by boring a small hole in the most dependent part of the gutter where the water stagnates. Heavy old metal cisterns, etc., not worth removal, can often be treated in the same manner.

(5). *The screening of breeding waters.*—We now proceed to consider methods for rendering localities or houses less suitable for mosquitos or for their breeding. We will deal first with what I call "minor works."

Mosquitos, more especially the *Culicines*, breed very much in collections of water, such as water-butts, cisterns, wells, cess-pits, etc., which cannot be dispensed with, and which should be so protected that the insects cannot lay their eggs in them. At the same time it is often necessary to ventilate the water. For this purpose we should apply covers which are so scrupulously well fitted that no insect can enter, and at the same time protect the ventilating orifice by wire-gauze. In many countries tubs are used outside houses to collect rain-water from the roof. A wooden top should be closely fitted and a hole of about 6 inches or more in diameter should be cut in it. Nail down a piece of wire-gauze on the outside of the orifice. The pipe which brings the water from the roof discharges over the orifice, so that the water flows directly into the barrel, being drawn out by a tap below. Similar devices can be employed for *cisterns*, but if these are made of metal and have badly fitting covers, considerable

difficulty is often experienced in making the proper arrangements, which must be left to the reader.

The subject of *wells* is of great importance in the tropics, as they are often the only source of drinking water. For the usual hygienic reasons the people should never be allowed to lower buckets and ropes into wells, as this frequently leads to serious defilement of the water. In my opinion every well should be furnished with a closely fitting cover, the water being drawn by means of a pump of some kind. But in order to ventilate the water, a hole protected by strong wire-gauze should be cut in the cover. I have seen excellent wells of this kind in several localities, notably at Bathurst, Gambia.

Cess-pits are frequently used, especially in French possessions, and are ventilated by long pipes which discharge at the roofs of the houses. We observed at Ismailia that *Culicines* were breeding in large numbers in these pits, though they were entirely closed except for the ventilating shafts, down which adults found entrance and exit. To prevent this it is necessary only to protect the ventilation with strong wire-gauze. In badly-made cess-pits there are other orifices by which the insects can enter. These should be done away with as much as possible. If they cannot be completely filled up, the pits must be regularly oiled by some method (section 53).

(6). *Dealing with breeding-holes.*—I have already mentioned that holes in trees and rocks should be filled with concrete, that is, with a mixture of Portland cement and sand or gravel. The best mixture for the locality should be determined by local engineers. Two men must work together, one carrying concrete and the other laying it into the holes. Mosquito breeding-holes in trees are sometimes very difficult to find, men or boys having to climb the trees for the purpose. Hence, when they are found their position should be marked on the tree by a conspicuous patch of paint for future identification. Inexperienced persons are apt to think that such work is altogether too much for human weakness to accomplish, but

as a couple of men can generally fill fifty holes a day, it is not long before most of the breeding-places round houses are disposed of. In many cases, moreover, the water in such holes can easily be liberated by cutting. If concrete is not available, earth, gravel, or stones often suffice perfectly for a considerable period. A little trouble guided by common sense is all that is required.

Garden cisterns often occasion much difficulty. Water is required for irrigating plants, and is often allowed to stagnate for weeks in the cisterns. The best way to deal with them is to direct that not too many of these cisterns be allowed to remain, so that those which are permitted to do so shall be in constant use. The frequent agitation of the water by the gardener generally suffices to check mosquito-breeding in it. The gardener should be instructed to deal with his irrigation water in such a manner as not to cause breeding of mosquitos. Here, again, details can be better left to the reader.

Irrigation pits are often employed for cultivation, and are found to contain Anophelines, which live especially in the presence of grass and vegetation occurring at the edge of the water. These are often difficult to deal with, as the people occasionally require the water. If possible, they should be filled in, but if this is not possible, we can often render them unsuitable for larvae by dragging out the weeds and "backing up" the margin of the pit by filling with large stones, the owners being instructed to prevent the new growth of vegetation. A gang of men will deal in this manner with a number of such pits in a day.

Borrow pits, that is, pits from which earth has been taken for building purposes, are found everywhere, and are not always easy to deal with. They may often be filled up with ordinary town rubbish, but this should be ultimately packed into them closely and covered with a layer of earth. In some places they can be filled up at greater expense with stones brought from a distance, if such are available. Where

they cannot be filled up, the best plan to deal with them is by the method just suggested for irrigation pits. Where no stones can be obtained, and where the water cannot be drained away, our only resource is to *deepen* the bed. Make the banks "steep-to" all round, and clear away all the vegetation, leaving the water as before. Such places are particularly suitable for the introduction of natural enemies of larvae; but they must be examined from time to time to see whether they are really free from mosquitos.

A number of small questions of this nature are apt to rise in connection with cultivation, and must be left to the ingenuity of the director of the works. Railway companies often cause much trouble by their borrow pits along the course of railways, and they continue to do so in spite of the protests of medical men. This is a point which should always be taken note of by the Government of the colony, who should insist upon the borrow pits being so made as to allow the water to run off. I know that in absolutely flat country this is difficult, if not impossible; but in such cases much can be done by making regular pits without marshy borders, and by insisting that these shall be kept free of vegetation, and shall be stocked with fish, etc.

Built conduits and drains.—By this I mean water-courses and drains which run through masonry. In the course of time the masonry is apt to wear away, thus leaving breeding-holes. These holes can be dealt with by rebuilding, or by filling with concrete.

(7). *Training the banks of streams, rivers and lakes.*—The word "training" is one used by engineers to signify rendering banks more regular or solid. Untrained streams and lakes are apt to spread over the ground beyond their margins, and to cause innumerable small breeding-pools, and the object of the training is to check this tendency. The director of the work must be guided by circumstances how this can best be done. In many places the water may be deepened, the earth

taken from the bottom being used to build up a steep bank. In other places large stones may be arranged along the margins. Everywhere the streams should be so dealt with as to allow the water to run freely, and above all, weeds should be dragged out of the water, while the banks are rendered as "clean" and permanent as possible. The Forest Department in Mauritius "canalised" a stream in this way, at the cost of only 0·37 rupees a running foot for both banks.[1] While the rough training is in progress, holes in neighbouring rocks may be filled in with cement. Cattle often cause much trouble by leaving holes in muddy banks in which Anophelines breed with readiness. This problem is attacked in Panama by a rule that cattle be allowed to water only at fixed spots which are properly prepared by cobble-stones to prevent such breeding.

Exactly similar measures are required for rivers and lakes, but the works involved for them must often be taken under the heading of major works. Irrigation canals and irrigation works in general must be attended to on the same principles.

It is often stated that irrigation is not compatible with mosquito reduction. This is quite untrue, because in Ismailia at the present day we have an example of the opposite. Ismailia, though built in the desert, is now a well-watered garden without mosquitos. Nothing but attention to the small details which I have mentioned is required, and the expense of the work is very small. Idleness always finds an excuse.

Small marshes can frequently be dealt with quite easily by these simple means, that is, merely by clearing the natural lines of drainage. Our mosquito gangs in Mauritius, consisting merely of Indian workmen, made short work of considerable areas of marsh in this manner. Only common sense is required; but, of course, in other instances major works by drainage have to be performed in order to give a final outlet to the water.

Temporary marshes (section 30 (1)) can be attacked on the same principles.

[1] See pp. 166 and 450.

(8). *Major works.*—I divide all works for the reduction of mosquitos into two classes, namely (*a*) such as we have hitherto dealt with, which can be performed by any intelligent persons, and should be called *minor works*; and (*b*) those which require the advice of an engineer, and which should be called *major works.* Naturally, major works generally require a greater expenditure of capital than minor works, though this does not imply that they are always more expensive in the long run. As a general rule, I think, major works need not be undertaken until minor works have failed, unless expert advice to the contrary is given.

Marshes are of two kinds, those in which the natural outlets for the water are sufficient but have become choked by vegetation, cultivation, or other small obstructions, which can usually be removed by minor works; and those in which the natural outlets are insufficient and require deepening. For adequate dealing with the latter, levels must be taken and extensive works, directed by an engineer, are generally demanded.

Marshes may be caused not only by insufficient outlet, but by excessive inlet. The water in a marsh may not come merely from rainfall over the marshy area, but from streams or drainage from beyond. Thus marshes are very apt to occur at the base of hills, small or large, the rainfall upon which runs out and stagnates on the plain. We must therefore consider whether we should increase the outlet or decrease the inlet; whether we should cut channels across a marsh in order to free the water in it, or carry intercepting trenches round it in order to remove the incoming waters.

In many cases, especially in marshes due to periodical inundations by streams and lakes, the problem of how best to deal with them can be solved only by an expert. The marsh may be (*a*) drained away, (*b*) filled up, or (*c*) converted into an open lake too deep to breed mosquitos. Estimates must be prepared, and the engineers have to select the best plan.

Marshes difficult to deal with are often formed by roads

railways, houses, irrigation canals, ill-managed water conduits and standpipes, and even by badly-made drains.

The form of drain to be employed often requires careful thought. Drains may be (*a*) open channels cut in the soil; (*b*) open channels flanked with stone or concrete; (*c*) channels filled with large stones at bottom and gravel above (rubble drains); or (*d*) various kinds of subsoil piping.

All major works require to be constantly kept in repair by minor works, that is, after the major work is done, there must be a constant expenditure for what engineers call "maintenance."

Though it may be quite possible to treat a given breeding surface by minor works only, yet the expense of this may be so great that it will exceed the interest on the money laid out for clearing the area by a major work *plus* the expense of maintenance.

In all these matters the advice of the local engineers should be sought. But this does not mean that the advice of the director of the anti-malarial campaign should be disregarded. They must work together. For instance, M. Watson notes (section 57), that open channels are quite effective in the lowlands of his district, because the local carrier will not breed in them; but piped drains are required in the uplands where the carriers breed in any open water-course.[1] Obviously the habits of the local carriers should always be carefully ascertained and considered, because this is likely to diminish largely the cost of their reduction.

Engineering works made in ignorance or in defiance of this knowledge may do more harm than good. For example, I think that most of the malaria in Freetown, Sierra Leone, was due to the badly-made drains; and I have seen much malaria caused by other works constructed without regard to their possible evil sanitary consequences.

A whole book could be written on drainage of the soil as a

[1] Compare sections 42 and 43.

preventive of malaria—the history of the subject alone would fill many pages. The reader will find further details in the next chapter.

(9). *Trees.* — Vegetation has long been known to affect malaria. In some places it seems to have a bad effect, and in others a good effect. Probably this depends largely upon the habits of the local carrier. We quite understand that certain mosquitos prefer wooded country, while others like the open. Thus M. Watson finds that in his district one of the most important measures consists simply in clearing the jungle to a considerable distance round habitations. On the other hand, it has been sometimes claimed that trees round habitations exclude mosquitos breeding in marshes beyond.

Suggestions were made some time ago that eucalyptus trees would have the effect of drying up the soil, and that their odour is inimical to mosquitos. Consequently many plantations of eucalyptus were made in Italy, Mauritius, India and elsewhere, but the results have been quite indefinite, as might be supposed. I certainly think that eucalyptus tends to dry up the soil, but whether it does so sufficiently to check mosquito-breeding is another matter. The cost of making plantations will generally exceed that of minor works.

It is well known that trees exhale a great amount of damp, which is always favourable to mosquitos and unfavourable to men. On the whole, then, I am strongly of opinion that many trees should not be allowed in the proximity of houses or in the middle of towns in the tropics. In addition to giving out moisture they exclude the breeze and increase the heat, because the amount of shade thrown by them scarcely compensates for the cooling effect of wind. In the case of houses surrounded by considerable grounds, trees are pleasant and beneficial at a distance from the house. As already mentioned, moreover, trees are almost sure to breed *Culicines* in holes in their trunks and principal branches. I therefore think that every tropical country should possess strict rules empowering the health

department to control the growth of trees within, say, 100 metres of houses. In Port Louis, Mauritius, the Mayor, Dr Laurent, very wisely made a great reduction in the forestation. In the same place much discussion has occurred regarding what are called the "river reserves." By an old law the forest department insists upon maintaining a belt of trees in the neighbourhood of each of the streams for the purpose, partly of preserving the streams, and partly of maintaining the rainfall, forests being found to increase the number of rainy days, a thing which is very useful for various kinds of cultivation. On consideration I was very doubtful as to the effect of such river reserves, but suggested that in the neighbourhood of towns and villages they should be kept scrupulously free from undergrowth.

As a general rule undergrowth is favourable to subdomestic and wild species of Anophelines, which, after leaving houses where they find their food, like to take refuge in thick vegetation. Personally, I prefer houses surrounded by open lawns rather than by thick shrubberies. In a house occupied by Major Fowler and myself in Mauritius, and surrounded by trees, the *Scutomyia notoscripta* was a pest by day, while *C. fatigans* was equally troublesome by night. In the barracks of the European troops, however, situated in an open grassy maidan a few hundred yards away, these insects were not nearly so numerous. Many similar experiences might be cited.

(10.) *Houses.*—Without making any general attack on the numbers of mosquitos in a locality, they can be reduced in dwellings simply by the manner in which the house is built. As a general rule Anophelines much prefer dark damp houses, especially with wooden or mud walls and thatched roofs, and surrounded by thick vegetation. Lofty houses built of stone, with large windows and plenty of ventilation, are not so haunted by Anophelines, though they may sometimes be full of *Culicines.*

I am sure that the colour of the walls is a very important detail. No mosquitos seem to like white walls and ceiling, especially if there is plenty of light (Nuttall [1901]).

Europeans called upon to live in the tropics are very apt to bring with them their ideas of house decoration, and English ladies especially like to furnish their drawing-rooms with numbers of beautiful curtains, pictures and stuffed chairs, all of which harbour the insects. This method of decoration is rather barbarous at the best, and is quite inappropriate for warm climates. Curtains especially check the breeze which is so cooling to the inmates and so unpleasant for mosquitos.

The whole subject of houses in the tropics requires very careful consideration. The housing of the poor is often simply disgraceful. The building laws, if any, are generally evaded, with the result that the mass of the people have to live in the most miserable shanties made of wood, palm leaves, and often of old kerosene oil tins. In every malarious country a committee should be appointed to consider the whole of this housing question, especially with a view to excluding mosquitos and malaria. Good houses suitable for the poor could easily be designed, and building laws could be made in order to enforce their construction by degrees. The same remarks apply to tropical cities, where the native quarters are generally in a dreadful condition. There is no reason which I can see why houses in the tropics should not be as well built as those in temperate climates. This, however, is a question of scientific administration in opposition to those forms of administration which we meet with too often in the world.

36. Prevention by Treatment.—It used to be said that prevention is better than cure, but we are now finding that cure is one of the best methods of prevention. In all parasitic diseases, one way to remedy the disease is to destroy the parasites in the patients, who then cease to spread them to other persons. This has been undoubtedly the case with

ankylostomiasis, and will prove to be the case with other parasitic diseases.

The history of prevention by treatment in malaria is a long one: Laveran [1907] gives many interesting details. After the mosquito theory was established, Koch was the first to call marked attention to this method. He suggested that all patients, especially children, should be carefully treated with quinine. His principles were immediately tried in Italy and in various German possessions (sections 49, 51); and in Professor Celli's contribution to this book, an admirable account will be found of the use of the method with more recent extensions given by the Italians themselves. Equally good results are quoted by other contributors to Chapter VII, where many practical details are given.

As stated in section 33, treatment affects the malaria equation partly by increasing the recovery factor r, and also by reducing the proportion of infected persons with gametids in their blood i. It thus modifies two of the factors. Treatment of the sick only should be called *case reduction*; and treatment of those who have not yet shown symptoms, but who may have become infected unknown to themselves, should be called *quinine prophylaxis*.

We should always understand that quinine does not really prevent infection. I presume that the protospores may be inoculated into a person whether he is taking quinine or not, but that they will not thrive in the former case—see negative cases 5 and 6 in section 15.[1] We should also understand from section 23 that quinine, whether used for case reduction or for prophylaxis, should be continued for a long time.

The drug does not have a very lethal effect on the gametids, at least those of the malignant parasites, when these are once formed; but it certainly tends toward destroying the other forms. The Italians insist that it does not have much effect upon relapses, but, for reasons given in section 23, I doubt

[1] And section 65 (5).

whether they are right on the point, and am inclined to think that the inadequacy of the treatment has been to blame for the recurrences of fever. This point is made clearer by some of our cases recently studied in Liverpool by enumerative methods. On the whole, there is no doubt whatever that a continuous and sufficient use of quinine will tend largely to reduce the malaria ratio.

Another point must not be forgotten. For public health work cases cannot be treated with the elaboration of detail employed in hospital practice. All we can do is to recommend a general line of treatment for the public; and, in fact, the people must become their own physicians. It is idle, therefore, under these circumstances, to suggest complicated scales of dosage and the use of various preparations.

I have always taught, in entire agreement with Professor Celli,[1] that daily dosage is much the best for public health work. He insists strongly that physiological immunity of the patients against the drug is much more quickly arrived at by daily dosage than by intermittent dosage—and I have always found the same. But there is a still stronger reason for daily dosage. If we order quinine every few days, the patient is apt to put off the evil hour until to-morrow—he complains that he has much work to do to-day, or that he has indigestion, or is not feeling very well. When to-morrow comes other excuses may be found. Then the patient is attacked with fever and finally decides that the quinine treatment is of no use. Personally, I recommend that quinine should be given invariably once a day, either just before the morning meal or at bedtime.

With regard to dosage, I do not advise quantities which will disgust the people with the drug. It is perhaps better to use amounts which will prevent the disease only in a percentage of people, than to cure all of them at the expense of making them all ill. Celli recommends 40 centigrams of the bisulphate, hydrochlorate, or bihydrochlorate for adults and young persons;

[1] See section 49, p. 417.

20 centigrams of the same salts or 30 centigrams of the tannate for children; and in every case administered in the agreeable form of comfits or of chocolates. But he increases the dose to 50 or 60 centigrams for adults in the presence of very severe malaria. I have generally recommended 5 grains (33 centigrams) for adults, and much similar doses for children. It has, of course, been shown that children can take quinine in larger doses than adults; but, on the other hand, I doubt whether such larger doses are really necessary for them, and the vomiting which is frequently caused in young children by the larger doses is apt to alarm mothers. In fact we have to take the greatest care not to impose too great a burden on the people whom we wish to persuade into anti-malarial courses.

In India quinine is generally administered for public use in the form of powders wrapped in papers. I think that this is a bad method, partly because the taste is so very apparent in powder, and also because much of it is wasted. I recommend freshly-made pills or tablets, coated in a suitable manner. If these are swallowed in milk or in water just before a meal, the taste of them does not remain in the mouth for more than a few seconds. Personally, I always take the drug thus in preference to chocolates, etc., in which the bitterness seems to me to be rather increased than diminished by contrast to the sweetness of the preparation. Small children can generally be bribed to take a pill by giving them some small sweet afterwards.

The experiments on the absorption and elimination of quinine (section 23) cannot be accepted as having answered the question as to which is the best salt for use in treatment or prevention. For this purpose accurate observations by enumerative methods, such as we are now adopting, must be carried out at length. The only answer to the question can be obtained by finding exactly how many parasites are killed by a given dose. I have usually recommended the more soluble salts, because a greater quantity is likely to be found in the blood at a given moment; the insoluble salts are certainly

absorbed very well, but probably not so suddenly as to produce a maximum poisoning effect on the parasites. On the other hand, the insoluble salts are likely to keep the blood more continuously impregnated with the drug, and may therefore be more useful for general prevention. Pending such researches, it cannot be said that one salt is much more efficacious than another; and the salts selected must depend upon the local price, and the local facilities for making up any forms useful for administration. The sulphate is certainly the most generally used in British possessions, but the Italians have lately insisted very strongly upon the tannate, which certainly has great advantages, being nearly tasteless and comparatively cheap. Euquinine is more expensive, and is generally prescribed in doses of 50 centigrams for adults and 25 centigrams for children.

Arsenic was used long ago for prophylaxis by Tommasi-Crudeli, but has now been abandoned in Italy. We are making further experiments with atoxyl and methylene blue.

It would be most useful if some drug could be found which affects crescents more definitely than quinine does.

Various other agents, including serum of cattle, have been tried, but the experiments will have to be repeated by enumerative methods.

Segregation.—On going to West Africa in 1899, after service in India, I was much struck by the fact that Europeans in the former country are not segregated as they are in the latter. In India the British officials and troops are nearly always housed in separate locations commonly called cantonments, so that they are frequently spared infection derived from neighbouring crowded native quarters. But in Freetown, Sierra Leone, the officials were obliged to live almost in the midst of the native population, even ladies being housed in rooms under which native shops existed. I called attention to this immediately, and attributed much of the large death-rate amongst Europeans to it. The idea was elaborated by Stephens and Christophers, who added the important consideration that native children are

the principal sources of the parasites, and J. E. Dutton strongly recommended segregation for Bathurst. Others opposed the idea, because they thought it would be wicked for the Europeans to live apart from their coloured brethren.

At this time the advisibility of reducing mosquitos was much disputed on the ground that the insects diffuse themselves in all directions from a breeding centre, just as particles of gas diffuse themselves from a generating bottle—which is, of course, not the case. At the same time segregation was much urged as an alternative measure; but I pointed out [1904] that both the measures depend upon the same principle—namely, that mosquitos do not diffuse themselves without limit from a centre. That is, I suggested the principles of random scatter given in section 29 (14). Obviously, if mosquitos spread to great distances, local drainage will not reduce their numbers, and, equally certainly, segregation will not enable us to avoid them. If this were the case, mosquitos infected in native villages would be able to travel to such distances that segregation would be useless. Happily, however, this is not the case, and therefore both segregation and mosquito reduction remain valid.

We may segregate ourselves from the proximity either of many infected persons or of breeding-places. S. P. James [1903] removed a number of people from a locality in which Anophelines abounded to one in which they were more scarce. In October 1901, seventy-eight children belonging to these people showed malaria parasites in 56%. In August 1902 they were all moved into tents about 600 metres from the nearest pool—no Anophelines now being found in the tents. The result was that not a single case of fever occurred among the adults, and that on 24th October the height of the fever season, parasites were found only in one out of twenty-five children examined — and this without treatment of any kind. Their spleen rate had also decreased from 75% in April 1902 to 60% in October 1902 — the splenomegaly evidently disappeared much more slowly than the parasite index.

As proof of the good effect of segregation from infected persons, I think that I may safely quote the case of the Indian cantonments, where malaria is certainly much less rife than in the crowded native quarters in the neighbourhood. Similar segregation has recently been carried out in Sierra Leone, where a special quarter has been provided for officials at some distance from Freetown—owing, I believe, largely to my advice given previously.

I look upon segregation as a most important measure for preserving the health of European officials, not only against malaria, but against many other diseases. It should always be adopted until sanitation in general arrives at a much higher degree of development in the tropics than it has hitherto attained.

37. Selection of Measures for Personal and Domestic Prophylaxis.—The various preventive measures have been described in the previous sections as briefly as possible, because much more information is given in the important contributions to this book contained in the following chapter. But we must now discuss exactly an important matter, namely, the relative values of these measures for use under various circumstances. The preventive measures themselves are now well known and are described in all the text-books; but mere description is not enough, as it is always difficult to employ every one of the measures, and both the individual and the health officer may be called upon to select the most feasible ones. We have to examine, first, the measures most appropriate for the individual—that is, for private persons, heads of houses, factories, institutions, ships, and so on; and, secondly, the much more complicated problem of the selection of methods for public prophylaxis.

(1). *Self-protection of Europeans in the tropics.*—I have had experience on this point during many years and in many different countries, and have only once been infected, and that at a time when we did not know how the infection is produced.

I always rely upon the possession of four articles, namely, a good bed-net, costing about eight shillings, a small hand-net for catching mosquitos, costing about one shilling, a palm leaf fan, costing one penny, and a bottle of quinine. With these articles the chances of becoming infected are reduced enormously —especially if a little common sense and care are added to the stock.

I cannot speak too highly for the ordinary bed-net, if used as directed in section 34 (1). Anophelines almost always bite at night. If the number of bites received by two individuals, one of whom always sleeps in a bed-net, while the other does not do so, could be strictly estimated, I think it would be found that the latter receives only about 10% of the number of bites inflicted upon the other. When a person is walking about in the day or in the evening he is not nearly so accessible to any species of mosquito as when he lies still and unconscious in sleep. It is therefore almost certain that the large majority of inoculations occur during sleep, and hence, obviously, protection during sleep will prevent a very large proportion of them.

Moreover, I have frequently found that persons who despise bed-nets, as many sportsmen, soldiers, planters and others are apt to do, are very prone to suffer from malaria. So are those who use bed-nets carelessly. After service in India, where most people are wise enough to guard themselves in this manner, I was much struck by the negligence on this point of Europeans in West Africa. There can be little doubt that the comparative good or bad health of Europeans in many parts of the world depends very much on the intelligence which they display in this matter—which seems such a small one to thoughtless people. Fortunately, the public have now been awakened, and I have even heard of several shipping and business firms who are wise enough to hold their employees responsible for becoming infected, unless they can show that they have used every precaution. This would be a very good plan to adopt generally, even by governments and military authorities.

Next to the bed-net, I attach most value to the small palm leaf hand-fan ; as stated above, I much prefer the use of it to thick gloves, mosquito-boots, etc. Here again the more manly, but often more unwise person, is apt to despise the suggestions of common sense.

The small hand-net is almost equally valuable. If I sit down to write or read, I am generally visited at once by a number of hungry suitors, and I like to take the precaution of catching and killing the lot before proceeding to my task. In fact I am not ashamed to confess that I take both a hand-net and a hand-fan to bed with me, in order to deal with any stray mosquito which may enter with me!

Protected by these humble means, I do not often have recourse to quinine. In most places in the tropics where large numbers of Europeans are called upon to live, the continuous use of quinine is scarcely demanded. For example, in the Indian cantonments, or the principal towns and settlements in other tropical countries, the chances of infection are too small to compensate for the discomforts caused by the drug, even when given in the best form and in the best way. It is, however, another matter when one is obliged to sleep in places of high endemicity ; and here we should always use the drug. Medical men can often be guided also by the great prevalence of Anophelines ; and in fact many of the general public are now beginning to recognise these dangerous insects by their attitude and the spots on their wings. If exposed in this manner, or if I think that I may have been bitten by infected mosquitos, I take the drug daily for about a fortnight in doses of between 5-10 grains (1/3 to 2/3 grammes). I know that this will not absolutely insure destruction of any protospores which may have entered, but I trust to luck that this has been the case. Anyway, I think that the inoculated germs have been so much reduced in numbers that (for certain reasons of my own) I think they are not likely to produce a very severe illness,

even if they do reach the fever limit. Then also I am otherwise protected by netting.

How far we should recommend what some may consider to be reprehensible carelessness for the general public is another question. The medical man does not always adopt for himself the advice which he gives to his patients; and I therefore prefer to leave further details under this head to the reader.

The European in the tropics must be told that he cannot insure himself against infection to an absolute certainty, however careful he may be. By bad luck he may be bitten when least expecting it, and by good luck he may escape altogether, just as the soldier may receive or escape the fatal bullet in battle. Other precautions remain to be mentioned. If we can select our place of residence, we should, of course, never live in proximity to native locations, or to marshes, or in old tumble-down thatched houses, or (very frequently) in dirty hotels. One does not therefore always welcome the hospitality of residents, however well meaning it may be. For travellers it is most important to recommend that they shall not go and live inside village houses; they should always endeavour to pitch tents, both for themselves and for their followers, in appropriate country at some distance. Of course travellers should always be provided with mosquito-proof tents, both for themselves and for their men.

(2). *Domestic precautions.* — The head of a house should enforce the various personal measures for the whole of his family on the lines just given. He should also try to screen a portion of his house. In many places this will be difficult, because in the tropics at least occupiers do not own their own houses and owners are generally averse to paying for such screening. But it does not cost much to screen a small corner of a veranda for sitting in.

Next to nets, I advocate the more general use of punkas or fans. A comparison of the life of Europeans in India with it in many other tropical countries will force us to admit their

great value. Malarial infection of Europeans is very rare in Indian cantonments, where they use punkas almost day and night, but it is common enough among planters, who are apt to neglect them. I really think that punkas prevent something like 50 to 70% of infection. Moreover, they keep the body cool and vigorous, while without them we remain covered with perspiration and tormented by mosquitos all day long. So convinced am I of the value of punkas or fans for general hygienic reasons that I would advocate the use of them even in places where every mosquito has been banished.

The question of prevention for *European children* in the tropics is of the greatest importance. The difficulty of rearing them is due, I think, partly to the great heat, partly to malaria, and partly to various entozoa and other diseases. Those who can afford screening and punkas should therefore employ them for their children at whatever cost. If they cannot afford them, they should not bring their children to the tropics. The habitual use of prophylactic quinine is seldom seen among European children in the tropics, and I doubt whether it can be generally recommended. In fact, if such use is actually required, the place should be considered too malarious for children. Of course it is another matter if children have already become infected.

(3). *Troops on service.*—The amount of baggage allowed is always very small, but the last article to be dispensed with should be the bed-net. In my opinion the military authorities should make all efforts to provide every man with one, because the cost due to loss of strength in consequence of invaliding from malaria must always be vastly in excess of that of the purchase and transport of nets. The disease has caused well-known disasters to troops, as in the British Walcheren expedition and the French expeditions in Madagascar (section 60). On the other hand, the American troops in the Philippines used nets and admitted their great value. Where the authorities fail in providing them the wise soldier will do well to follow the old maxim of "self-help." A daily quinine ration of say 5 grains

every morning should also be insisted upon, together with the rigorous use of nets. I scarcely think that gloves and veils, such as Laveran figures as used among the Japanese troops, are so urgently demanded—at least in the tropics, where they would be nearly intolerable. The Medical Department has the care of choice of proper bivouacs and quarters.[1]

(4). *Ships.*—I always carry a net which can, if required, be hung over the bunk. Ships in the tropics often delay for hours or days in port, and are frequently visited by swarms of mosquitos. Of course, all ships owned by companies who possess any degree of humanity and intelligence should be invariably screened against mosquitos in the manner shown by Dr Wolferstan Thomas (section 46).

Quinine should be always given to the crew on arrival at the first malarious port, and for at least one month after leaving the last one, if the voyage lasts so long. The great mistake of stopping the drug on departure from the malarious coast is often, indeed generally, made. Many of our nautical patients in Liverpool give histories of having been attacked for the first time when nearing the British coast—that is, their infections had been received weeks previously, but had been suppressed by the drug until the use of it was abandoned.

(5). *Hospitals and asylums.*—Here quinine cannot be given without interfering with the treatment of the various kinds of disease present. The superintendent must see to it that mosquitos are not being bred within the hospital premises; but unless mosquito reduction has been adopted for a considerable radius outside the institution, screening should be adopted as well. Hospitals with crowded, unprotected wards furnish the worst examples of congregate sleeping (section 28 (3)), so favourable to the mosquito transference of several diseases. For example, dengue prevailed greatly in an unprotected hospital in the Philippines, while it was absent from a protected one; and I have frequently found filariae and

[1] See destruction of adult mosquitos, p. 362.

plasmodia in hospital mosquitos. In fact the dangers are so great that it is perhaps better to advise that every hospital should be screened whether mosquito reduction is carried out or not. A mosquito-infested hospital is a most disgraceful thing. The miserable defenceless people it contains are infected in the very place where they have come to be cured, and there is no excuse for it.

(6). *Barracks.*—Unless mosquito reduction can be rendered very thorough, the best plan is perhaps to employ screening as well. Probably most of the inoculations occur at night, largely in consequence of the congregate sleeping usually adopted in barracks. The cost of death and invaliding of soldiers is probably far in excess of that of screening. Bed-nets are now being often used or advocated, even for native soldiers; and I have heard of several cases where officers have helped their men to purchase them. But surely it is the duty of the military authorities to provide screening instead. Enormous sums are frequently wasted on the erection of new barracks—sums which would have sufficed to screen all the barracks in a country. The neglect on this point is simply marvellous. In one case, known to me personally, a serious epidemic of malaria occurred because the authorities were disputing as to who should pay for the hooks required to hang the bed-nets (already purchased!). Several of the men died, and a number were invalided at a cost which afterwards sufficed to drain the whole neighbourhood.

Punkas pulled by machinery have long been in use in many military hospitals and barracks in India.

Unless screening is very perfect, great care must be taken to complete the extermination of the parasites in infected soldiers. In any case this is necessary in order to maintain the effective fighting strength, because soldiers "soaked" with malaria are only a source of expense in war. I doubt whether the six weeks' treatment used in India is quite sufficient.[1]

Of course a large proportion of the infections among troops

[1] See H. H. Johnston, *Brit. Med. Journ.*, 15th August 1908.

are often contracted outside barracks. The question then becomes one of public prophylaxis (section 39).

The quarters of officers and families should, in my opinion, be always screened unless mosquito reduction is very perfect.

In the old days, in British possessions, large areas round barracks were kept strictly under the cantonment authorities, who refused to allow the general populace to live in them. Unfortunately, owing to the pernicious sentimental outcries which are proving so destructive to vigour and prosperity, this wise rule is now tending to fall out of use; filthy huts spring up close to the barracks; the troops become heavily infected with malaria and other diseases; and the nation which gives way to this brainless folly has to pay much more for its military service, and may have to pay still more, some day, for military defeat.

(7). *Prisons.* — The prisoners are taken from the lowest populace, and are being constantly changed as sentences expire. Hence large numbers of them are infected outside, and the principal measure is case reduction. In addition to this, screening or mosquito reduction, or both, should be adopted according to circumstances.

(8). *Schools.* — Boarding schools are scarce in malarious countries. Day schools give a most valuable opportunity, not only for dealing with malaria by case reduction, but also for measuring it; but this matter is part of the general plan of campaign (section 40 (6)).

(9). *General rules.*—In all matters of domestic prophylaxis it is wise to remember the following rules (which cannot be repeated too often):—(*a*) unless there is strong *prima facie* reason to believe that cheap measures must fail, always begin with them before attempting more expensive ones; and (*b*) do not adopt any measure until careful examination has proved the necessity for it. For example, I have seen and heard of expensive screening being installed (*a*) where a little mosquito brigade work would have cleared the mosquitos entirely, and (*b*) where most of the infections were really being acquired outside.

38. General Sanitary Axioms. — Before we consider the complex subject of public malaria prevention, it is advisable to state the following series of propositions regarding the prevention of all diseases.

(1). Widespread diseases, especially endemic diseases, cause much pain, sorrow, expense and loss of prosperity to the people.

(2). Next to the maintenance of the State, it is the duty of scientific governments to investigate their mode of propagation, and to endeavour to control them.

(3). For economic reasons alone, governments are justified in spending for the prevention of such diseases a sum of money equal to the loss which the diseases inflict upon the people.

(4). The amount of money spent on the prevention of various diseases should, *ceteris paribus*, be proportioned to the amount of sickness and mortality caused by each.

(5). It is the duty of governments to make and to enforce ordinances required for the prevention of diseases; and of the people to submit to them.

(6). *Ceteris paribus*, that sanitary measure is the wisest which causes the public the least inconvenience.

(7). *Ceteris paribus*, that measure is the most practicable which can be carried out by governments without making any demands at all on the thoughts, efforts or compliance of private persons.

(8). *Ceteris paribus*, that measure is the most economical which confers, for unit of cost, the widest benefits on the public.

(9). For the prevention of diseases on a large scale a suitable expert organisation is always required.

(10). It is always advisable to carry out accurate and repeated measurements of the prevalence of the disease which we propose to prevent; of the cost of the adopted measures and of their results.

Written out in this manner these statements appear trite enough ; but in my experience every one of them is forgotten in actual administration. Thus few persons remember the first axiom—until they themselves are taken ill. Few governments really attend to the second one. Scarcely any one has conceived the simple idea contained in the third, and it is the rule to grudge spending a hundred pounds for a disease which costs thousands. Dramatic diseases interest many, while common ones are forgotten ; and the remaining axioms are too often overlooked even by those who direct or discuss sanitary work.

39. Selection of Measures for Public Prevention.—From the fundamental reasoning in sections 28 and 33, and also from general experience in many countries during the last ten years, we are now probably justified in making the following statements :—That malaria can be completely extirpated in a locality by the complete adoption of any one of the three great preventive measures, namely, *protection*, *mosquito reduction*, and *treatment*. That is to say, if every person in the community could be fully guarded against mosquito bites, or if every Anopheline could be banished, or if every infected person could be thoroughly treated from the beginning of his case, then each of these measures ought to suffice by itself to banish the disease entirely. Moreover, if we could use two of the measures, or all of them, the result would be still more assured.

But, obviously, it will never be possible in any general community to adopt or to enforce any one of these measures *completely*. Thus, however carefully mosquito-nets or screening are used, many people will still continue to be bitten occasionally, so long as the mosquitos are allowed to remain. Again, however carefully we may destroy every mosquito which we can catch, or every larva, or remove the principal breeding-places, still, a few insects and breeding-places are sure to escape us. Thirdly, however strongly we may urge suitable treatment of all infected persons, a few are sure to resist it, and to

spread the disease if any Anophelines are left. Hence, though theoretically possible, the *complete* use of any one of the measures is not really practicable.

Fortunately, however, we can see by careful consideration of the principles laid down in section 28 that complete eradication of malaria may still be possible by the adoption of each measure by itself, even if that measure is not completely given effect to. By that section we see that the static malaria (section 30) will tend to vanish, not only if all the factors of the equation are rendered absolutely prohibitive, but also if they are reduced to a certain figure ; that is, if the new infections can no longer keep pace with the natural recoveries. This important and encouraging law has been well exemplified in Great Britain, from which the disease has entirely disappeared owing to partial and indeed almost unconscious agencies, as described in section 30 (21).

Lastly, we can understand from the same reasoning that a partial adoption of any one of the measures, though it may not banish malaria absolutely, is still likely, or indeed certain, to make some reduction in the disease provided that the other factors remain unchanged.

All these truths still continue to apply if we adopt not one single measure, but several combined. Thus it may well be that an extremely partial application of two of the measures will produce results as good as a less partial application of one of them would have done ; and this fact may help us to do the work much more economically.

Thus the head of a sanitary department who intends to carry out a large campaign is fortunately able to avail himself of several measures or combination of measures, by any one of which his purpose may be effected. But he has to consider means as well as objects : his duty to his Government demands that he should spend as little money as possible, and his duty to the people demands that he should not trouble them too much. Still more important, the various measures are more or

less practicable according to a number of local conditions, all of which must be carefully considered by him before he comes to a decision. In practical sanitary politics, therefore, the mere enumeration and general description of the various measures are quite insufficient, and we are obliged to discuss with great care their relative values under different circumstances. The reader can assure himself still further of this, by studying the numerous contributions given by the most eminent men in the following chapter. He will see that different measures are being adopted in different countries. In some countries, especially Panama, Egypt, Durban and the Federated Malay States, mosquito reduction is the principal measure. In others, especially Italy, Greece and the German and French possessions, prevention by treatment is the favourite remedy ; and in other places combined measures are in use. In fact there has been much discussion upon this point during the last ten years ; some urge one measure and some urge others—each being guided by his own experience in his own malarious country. The truth is that all the measures are good and useful, and that each is most suitable under certain circumstances. The subject is evidently a complex one. I will try to make the issues as clear as possible by supposing at first that each measure is to be adopted by itself alone.

Protection (section 34) requires the use of bed-nets, screening, fans and some other devices. It demands an entire concurrence of the people who are to be protected. Probably no government can force the public to protect themselves. Although the rich may be quite willing to do so by bed-nets or screening, the poor will generally refuse the expense incurred. The cost of a good bed-net is, for instance, equal to the monthly pay of a day labourer in India. The cost of screening a house completely is often more than a tenant cares to incur, and the house owner generally refuses to undertake it for tenants who occupy their houses on short leases (as generally happens in the tropics). Punkas and fans are scarcely

practicable for the poor, and medicinal culicifuges are of little value. In most countries the poor, especially in rural districts, are badly housed in huts which are everywhere permeable to mosquitos. To protect such houses by any known means will generally cost as much as rebuilding the whole house. Even if the state would be willing to provide every householder with a bed-net for himself and his family, it would still have to make arrangements for the constant repair of them, while they would often remain unused. We can scarcely expect that poor families, living crowded together in a single chamber, will ever exert themselves to use mechanical protection in a careful way. But even with the most complete protection of houses, people will still be subject to being bitten out-of-doors.

This, however, is to look at the subject only from the darker side. As already stated, even a partial protection is quite likely to have an immense effect on the malaria. Even if only a proportion of the people could be persuaded to use any care in protection, doubtless the new infections would be reduced so much that the static malaria might either disappear entirely or become very small. It is, therefore, most advisable to urge protection as strongly as possible; to advocate suitable housing for the poor; and to post notices for the information of the public regarding the dangers of being bitten by mosquitos.

We see, then, that the measure of general protection against mosquitos is scarcely capable of more than a very partial adoption by the public. As a Government measure it is opposed to the seventh sanitary axiom given above. Although it possesses what some governments may consider to be an advantage, namely, that it does not make direct demands upon their own finances, it may yet be in the end more costly to the public than other measures. In most countries the number of houses is equal to about 1/4 or 1/5 of the total population—that is to say, the houses are very numerous. The cost of screening each house or of providing all the innates with mosquito-nets will therefore mount up to a very large sum. Thus in a city

of 30,000 inhabitants, there are likely to be 6,000 houses, and if the average cost of screening each house amounts to only £1, the total cost, including that of maintenance, will be very large. I doubt whether any form of screening for the poor will not depreciate at the rate of 50% per annum. It may therefore happen that the total cost will greatly exceed that of mosquito reduction or treatment. Even if it does not exceed that of mosquito reduction, the advantages of the latter are so great that it would be preferable to adopt it (if it is feasible).

So much for protection as a *general* measure; but it is often decidedly called for under special circumstances, as for isolated houses in the country, especially in proximity to marshes; and for hospitals, barracks, prisons, hotels, travellers' rest-houses, tents, ships, river-boats, and so on.

For general prophylaxis, especially in crowded localities, *Anopheline reduction* has many great advantages, which I have often pointed out. Practically, Anopheline reduction may be generally merged into the wider measure of *mosquito reduction*, which costs little more to effect and adds greatly to the benefits obtained. It is essentially the radical measure, which tends to remove not only the carriers of the disease but also a continual source of annoyance to the public. As mosquitos are known to carry other diseases, namely, yellow fever, filariasis, and almost certainly dengue, mosquito reduction will probably do much more even than to reduce malaria. To the practical hygienist it has still greater claims for consideration, especially because it conforms perfectly, not only with the eighth sanitary axiom, but also with the sixth and seventh—which is not the case with some of the other measures. For example, a government or municipality can maintain men for the purpose of removing breeding-places without causing any trouble at all to the populace. A certain amount of legislation is required to control obstinate people who sometimes refuse out of wilfulness to allow sanitary agents into their premises; but beyond this it is generally advisable for the autnorities to do everything as

regards mosquito reduction for themselves. No one is asked to believe in the mosquito theory, or to take drugs, or to protect himself and his family with bed-nets, or to put screens to his windows. Nor does mosquito reduction cause much or any inconvenience to the public (axiom 6). Occasionally cultivation in proximity to towns must be controlled, and pits, cisterns, wells, etc., used for irrigation must be dealt with ; but compensation for any harm done must be paid for by the authorities, and, where mosquito reduction is generally feasible, only a small proportion of the people are affected in such matters. Lastly, mosquito reduction has the advantage (which is not always recognised) that it compels the sanitary authorities always to maintain a minute inspection of the area under their care, compels cleaning of all premises, removal of waste water, proper maintenance of roads, and so on (section 35).

Practically, however, questions of cost and feasibility have to be considered. It is almost impossible to give an estimate of the cost of mosquito reduction, which may depend everywhere on the nature of the soil, the slope of the ground, the rainfall, the vegetation, the existence of marshes, the character of the drainage and of the sanitary department already in use, and so on. Quite possibly, although the necessary expenditure is really within the capacity of the local budget, the authorities may not think so at the moment, so that the health officer is left powerless to adopt this measure. The advantage of mosquito reduction, that it can be carried out entirely by the state, has a corresponding disadvantage, namely, that the state imagines that it alone pays for the work. In a perfect governmental machine, the state could immediately call for increased taxes in consequence of any work of absolute public necessity; but in this very imperfect world, the public is apt to be rather blind in these matters, and the state is afraid of arousing hostility, even by saving people's lives. At the same time it is proper to note over and over again that mosquito reduction often really does not cost as much as some pretend. The

figures for Ismailia and the Federated Malay States include large items in excess of the actual anti-mosquito campaign, that is, items for general drainage, for building sites or cultivation purposes ; and those for Panama are so mixed that we cannot exactly extricate the cost of this measure by itself.[1] Moreover, by the general principles of section 28, malaria may be completely reduced even by a partial reduction of the Anophelines below the necessary limit (which I have roughly estimated to be about 40 different Anophelines per head per month) ; and it may be partially reduced even by a smaller reduction of the insects. In fact the small measures under the head of minor works generally cost but very little, and though they may not suffice for complete mosquito reduction, are still likely to do much good, especially if combined with other precautions.

Prevention by treatment also has many advantages. In every locality, even if every mosquito can be banished, a number of cases are likely to continue to suffer from relapses for many months, and these always require careful treatment. Hence case reduction should always be adopted as much as possible, whether the other measures are used or not. Such case reduction can hardly come within the sixth and seventh sanitary axiom, because no inconvenience or trouble is inflicted on the people by gratuitous treatment of their sickness. Another great advantage is that the measure can be commenced, even on a somewhat large scale, by medical men, without making any immediate large demands on the public purse. As shown in Italy and Greece, the mere sale of quinine at cost price by Government produces an excellent effect, if the people are intelligent enough to use it. Moreover, medical men who are now best acquainted with the whole theory of malaria, are precisely those who carry out this measure. In fact their practice actually receives some benefit by it, at least at first ; they are accustomed to the use of drugs, and they find no difficulty in urging their patients to take quinine.

[1] Col. Gorgas says $2·0 a head (section 42).

Unfortunately, all this does not apply in many countries in which the number of qualified medical men is very small. In many other countries the natives object entirely to our forms of treatment, and rebel even against quinine. Still further, many people are afraid of admitting the existence of domestic illness amongst their children, or will not take the trouble to treat them at all. Others, again, refuse to buy quinine, even at the cheapest rates, or cannot afford the time to attend hospitals or dispensaries for it, so that inefficient doses are frequently taken, with the result that the patients become sceptics as to the value of the drug, and advise their friends against it. In fact, these practical difficulties are so great that, as I stated in my Mauritius report, we are obliged in the tropics to distribute the drug gratis from house to house if we wish to obtain any material benefit from it. Lastly, if we propose to confine ourselves only to case reduction, that is, to the treatment of the sick, we have to distinguish the sick from the healthy, which generally can be done only by medical men, or by qualified assistants at the cost of considerable and continued labour.

This trouble is saved if the public can be persuaded to adopt not only case reduction, but also *quinine prophylaxis*—that is, the treatment of persons who have not yet shown signs of illness, but may possibly have become infected. Unfortunately, if there are difficulties in the way of treating the sick, there are sure to be still greater difficulties in the way of persuading healthy persons to take any form of quinine continuously. Poor and ignorant natives, and indeed many other people, object strongly to this course. Some say that they would actually prefer the occasional chance of malaria to a continued certainty of dyspepsia. It is true that if quinine is given daily in small doses it produces much less physiological effect; but in the majority of persons it always does produce some slight inconvenience. Busy people, hard-worked labourers or mothers of families tend to rebel very much against any continued

medication. We know in Europe how much people are apt to protest against vaccination—a single simple operation causing scarcely any discomfort, and protecting for years against a dangerous and disfiguring disease; how much more, therefore, will people be apt to protest against a medicine which must be taken daily for months during the malaria season, even if they are not affected by the disease. Of course, persons who remain infected, specially children, always continue to be a danger to their neighbours, and for this reason alone a complete reduction of malaria by treatment must always be difficult, unless the authorities have such power that they can actually force the drug down the throats of the people. This is often possible in the case of soldiers, employees of companies, or coolies on plantations, but scarcely for the general public.

On the other hand, we may rest assured, though complete case reduction is seldom practicable, that the partial measure is certain to do much good. Because we cannot force all the people to take quinine, that is no reason why we should not persuade as many as possible to do so.

Segregation, either from marshes or from infected persons, should not be looked upon as a measure of general public prevention, as it is applicable only in a few cases.

Next, let us compare the measures two and two—at the risk of repetition. First, suppose that we have to deal with an ideal country where all the measures are equally feasible, and will cost the same sum of money—a thing which, of course, never happens.

Protection is evidently at a disadvantage compared with mosquito reduction. It will never entirely prevent the bites of mosquitos; nets and screens always require some trouble to keep in order, and certainly add somewhat to the heat—often so unbearable in the tropics. On the other hand, mosquito reduction, if sufficient, removes at once an incubus, a veritable plague—we breathe, move and sleep freely, our children are safe, our verandas and gardens open to us.

Now, compare protection with prevention by quinine. The former guards us not only against all mosquito-borne diseases, but against constant personal annoyance ; the latter, only against malaria. Nets and screens cause no dyspepsia, no ringing in the ears, and no daily trouble with the children. I think that every one will prefer freedom both from mosquitos and from dyspepsia to the inflictions of both. Hence protection has great advantages. But, in spite of this, the sick must still be treated.

Mosquito reduction has all the advantages of protection and more, and is therefore greatly preferable (under our hypothetical conditions) to quinine. But, nevertheless, the sick must still be treated.

Ideally, therefore, mosquito reduction is by far the best measure. Protection is next, and quinine last.

Now, let us consider the measure from the point of view of cost and feasibility. The cost of protection must obviously vary according to the number of houses, which can be roughly ascertained by dividing the population by about four or five. In Mauritius at the census of 1901 the population was 373,336, and there were 93,031 houses, so that there were about 4·25 persons to each house on the average. Probably the cost of protecting a single house there would be quite £1 on the average, as a very low estimate, amounting to £93,031 (about 1,500,000 rupees) for the whole island. Besides this, repairs and renewals of the screens and nets would probably amount to quite 25% of the original cost at a low estimate; so that after the first installation of the protection an annual cost of £23,000 (350,000 rupees) would have to be incurred for the mechanical work of supervision. This, of course, is a very rough estimate, and quite possibly more than £1 would be required to protect each house on the average, because many of the poorest houses are little better than shelters of mud and thatch. Regarding the feasibility of protection, I repeat that it cannot be forced by Government on the people except as

regards employees of planters, railway companies, etc. Officials cannot, therefore, have any resource except to persuade and to keep on persuading, and even then only a proportion of the people would adopt the measures. For anything like general protection the Government would probably have to supply most of the cost—which we have seen will be very considerable. Of course, in Europe and the United States, where the people have reached a higher degree of prosperity than in most tropical countries, this measure is more likely to be adopted by the general public; but even in them the very poor and the agricultural classes are likely to cause great difficulties. It can, therefore, scarcely be said that general protection, or even protection of a percentage of houses, is an easy and cheap measure. Observe, also, that the cost of it is likely to vary according to the *number of houses.*

Notes on the cost of mosquito reduction have just been made. For Mauritius, which contains 705 square miles (1,924 square kilometres) and a population of 534 persons to 1 square mile, I estimated that a general mosquito reduction in inhabited areas, especially in towns, would cost, for minor works alone, the sum of £5,600 (83,910 rupees) per annum. For making this estimate I relied upon figures given to me by the Medical Department, who suggested the number of workmen which would be required for each village, plantation, town and district, and the sum includes funds for the director, and travelling expenses. But it does not include the cost of the major measures, that is, the drainage of large marshes, and dealing with such waters as require the advice of an engineer (section 35 (8)). This cost was estimated in Mauritius by the Public Works Department at a capital expenditure of about £42,000 (630,000 rupees), but I advised that these major works should be undertaken only where the minor works might be found, after experience, to be ineffective.

For the reasons already given it is almost impossible to furnish any general estimate for mosquito reduction anywhere.

The cost depends upon the number of major works required, the price of labour, the price of expert supervision, the local laws, and the local efficiency of administration. It is very difficult to extricate from the accounts of various campaigns the money expended on mosquito reduction alone. In the town of Port Said, however, where there is very little rainfall and where a complete Culicine reduction has been carried out, the cost has been about 5d. per head of population per annum (section 53).

I have already stated that mosquito reduction has the great advantage that it can be carried out by Government alone without reference to the people. Labourers can be employed to treat Anopheline waters or to clear away Culicine waters from the backyards of houses, without causing any real trouble to the public. It is not necessary for Government officials to go about persuading householders to do this or that, or for medical men to examine large numbers of individuals, and so on. On the other hand, mosquito reduction may just possibly be essentially unfeasible in places where the breeding waters are such that they cannot be removed for any cost that can be afforded by the local funds; and this is, of course, a question upon which it is impossible to make general statements.

As a broad rule the cost of mosquito reduction must always tend to vary according to the *area* of the country treated.

It is often thought, especially by medical men, that the cost of *prevention* by *treatment* is almost *nil.* We write prescriptions, which cost us nothing and which are made up by the dispenser; and we often seem to think that a similar prescription can be given, at the same small cost, to a whole nation. But the cost is sure to be very considerable. For Mauritius, in addition to the mosquito reduction, I prescribed a very partial scheme of prevention by treatment, costing 30,000 rupees for the quinine alone, 3,600 rupees for its preparation and despatch, and 6,000 rupees for the services of five quinine dispensers, amounting to £2,640, or 39,600 rupees, per annum.

This was *without* the salary of a director and cost of office, etc., and it was proposed that the quinine should be distributed gratis, but chiefly in intensely infected localities.

Here again the cost must always depend largely upon the local conditions. A highly civilised and prosperous people will take the drug readily, where a very ignorant, poor, coloured population will require the utmost amount of persuasion. Thus we have to pay not only for the amount of the drug used, and for its preparation and despatch, but also for an agency which will distribute it to the people and will persuade them to use it properly. We are very apt to forget this last and very costly item. Think for a moment of the position of the very poor in malarious villages and towns. The occupant of each house is generally obliged to go to his labour extremely early in the morning. His wife has her household duties and her children to attend to. What time have they to obtain the drug at local dispensaries which may perhaps be miles distant; and, very often, what money have they to purchase it near at hand? Moreover, neither the man nor the woman has time to force the medicine down the throats of the younger children. The same difficulties recur day after day and week after week, perhaps during the whole of the malaria season, and even beyond it. Meanwhile patients rebel against the medicine, just as much as those who care for them rebel against the necessity of fetching it, and soon there is very apt to be a general rebellion. This has been strongly felt already in many localities, where prevention by treatment has been much practised; and in fact it was for this reason that my advice was sought both at Ismailia and in Mauritius. Dr Malcolm Watson has also made some important remarks under this heading (section 57).

In my Mauritius report I laid down that for most tropical countries the only effective way of giving quinine is (*a*) to give it gratis, (*b*) to give sufficient quantities to last each patient for one or two weeks or more, and (*c*) to distribute it actually from house to house. Now all this must cost the state a very

considerable sum. As a matter of fact a large number of fully-qualified medical men, and also qualified assistants, must be employed to carry out this distribution ; and we must remember that the salary of a fully qualified medical man is as much as the wages of about fifty coolies (at least in India), and the salary of a reliable qualified assistant may amount to as much as the cost of five to ten coolies.

Regarding the feasibility, independent of cost, we must again remark that this measure does not accord with sanitary axioms 6 and 7.

The cost will tend to vary according to *number of population.*

To sum up—protection is evidently a personal or domestic measure which cannot be forced by any government on the people, unless the former is willing to supply the nets or screens to the poor—the cost of which would probably largely exceed that of mosquito reduction, at least in towns. But protection should always be adopted for public buildings and isolated houses, such as railway stations, rest-houses, and houses on estates. I think that it might also be used, even at the cost of supplying netting, for villages in proximity to large marshes which cannot be drained without great expense. At the same time it should always be urged on the public by means of lectures, pamphlets and placards, which cost Government almost nothing, and certainly have a good effect.

Practically, therefore, for public prophylaxis we are called upon to choose between mosquito reduction and quinine. Ideally the advantages of mosquito reduction are simply overwhelming — so much so that I urge *it should be universally adopted in all towns in the tropics, as a part of a general crusade against vermin of every description.* The relief caused by removal of the mosquito pest is great enough to compensate for the small cost involved (in towns) even if the insects do not carry any disease.

Practically, however, our choice is often limited by the financial position of the place and time. To familiarise him-

self with the question the reader will do well to recall to his memory various localities which are well known to him. Suppose, for illustration, that malaria were to break out in London round Hyde Park in consequence of an obstruction to the drainage to the Serpentine river. Can we suppose for a moment that the authorities would not at once undertake Anopheline reduction by removing this obstruction? Would not this be much cheaper and almost infinitely more convenient than to force people to screen their houses or to take quinine, while the hypothetical mosquitos are allowed to remain as numerous as before? Precisely the same answer must be given in the case of most cities, towns and even large villages in the tropics. But now take another extreme case, and suppose that the disease were to break out in any large rural area in Europe, such as among the bogs and lakelets of Connemara. What practical hygienist would urge mosquito reduction here, with the immense cost involved, merely for the benefit of a few scattered farm-houses. The appropriate measures would obviously be protection and quinine, as a general rule; but even here, we might occasionally find villages in which the Anophelines breed in a few local waters which can be removed at a smaller expense than would be involved in the other measures. In fact, between the two extreme cases suggested, there are innumerable instances where either of the measures or both may be used, and where the director must use his own judgment.

The cost of reduction of Anophelines tends to vary with area, and not with population. It does not matter much whether the area is crowded with houses or not; though, if anything, density of population, necessitating well-paved streets and the removal of agriculture, will tend to cheapen the cost per unit of area. Thus roughly, the cost of Anopheline reduction for a square mile of densely-crowded city or town is likely to be actually less than that of the Anopheline reduction for a square mile of uninhabited wilderness. In the former

case the expense will benefit a large number of human beings, and in the latter case only the birds and beasts.

On the other hand, the cost of case reduction and quinine prophylaxis must vary directly, not with area, but with population. It will be vastly greater in a crowded square mile than in a square mile containing only a few cottages. Therefore evidently mosquito reduction is called for by every consideration in the case of crowded areas, and, on the other hand, quinine tends to be the more proper measure amongst a scattered population, where Anopheline reduction would be too expensive for the local funds. This is, of course, a general rule, exceptions to which must be carefully considered by the director himself.

Much thoughtless matter has been written on this point. For instance, it has been stated that because quinine has proved so beneficial in Italy, it is also the appropriate measure for all malarious countries. But the success of quinine there does not prove that mosquito reduction might not have been equally successful, possibly even at the same cost. I do not say that this is the case, but it might be so. Moreover, the conditions in Italy and elsewhere may be extremely different. Italy possesses a temperate climate which admits of mosquito-breeding only during a few months of the year, so that there is plenty of time for the treatment of old cases during the non-malarious months. Italy is also a highly civilised country with an intelligent white population, amongst whom there are numbers of medical men who speak their own language, and who add to their practice by the treatment of cases of malaria. Lastly, in Italy malaria occurs principally in rural areas, that is just where quinine is rationally called for. What more natural than prevention by treatment in it. But in many other countries these conditions do not obtain ; malaria abounds in towns ; the population is ignorant and often stupid ; there are very few qualified medical men ; infections occur nearly all the year round ; and the cost of maintaining medical men and

dispensers for quinine distribution may, in urban areas, far exceed that, not only of Anopheline reduction, but of mosquito reduction in general. We really must not apply rules suitable for one country to all other countries. We have to consider each case on its own merits.

Protection is a measure for individuals, mosquito reduction for local governments and municipalities, guided by their sanitary departments, and prevention by treatment for doctors. It is not to be wondered at that the last named are so fond of the last measure; they are acquainted with drug giving, but often not acquainted with public health matters, drainage and general municipal polity. They naturally tend to select quinine, and often very wisely so. In many cases, however, it would have been a wiser policy to have adopted the larger measure from the first.

Up to the present we have considered each measure separately; but in many cases, probably in nearly all, it may be better to adopt a combination of several measures partially carried out. In fact, practically it nearly always comes to this. As previously stated, the complete adoption of any one measure is generally impossible; we cannot remove every mosquito, nor can we treat every case. Possibly a partial mosquito reduction combined with a partial case reduction will produce the same amount of malaria reduction at much less cost. In other words, we remove what breeding waters we can remove without great expense, and we treat as many of the infected persons as possible. At the same time we urge protection upon the populace and adopt various methods under different local conditions. The proper policy is not the protection policy, nor the mosquito reduction policy, nor the quinine policy, but an opportunist policy which uses any weapon it can. It is sometimes stated that this opportunist policy will cost more than the specialised policy; but this does not follow at all. Ultimately we have to frame our measures according to local feasibility.

40. Conduct of the Campaign.—Up to the present most of the anti-malaria campaigns have been due to the intelligence and energy of individuals; but it must be the desire of all—and certainly this book has been written in the hope of it—that similar work should now be attempted in every civilised malarious country. This has not hitherto been done, because often neither governments nor health departments have known how to commence and to organise their efforts. I propose now to make some suggestions on the point. They are based upon the scheme proposed by me for Mauritius, after careful consultation with the capable medical profession of that colony. They are tentative suggestions, and alterations of detail may be required under other conditions, and even in Mauritius; but I hope that, with such alterations, the general scheme will prove applicable to most malarious countries, especially in the tropics.

(1). *First steps.*—Whether the campaign is to be a large or a small one, the authorities must begin by educating not the public, but themselves, regarding the simple but often ignored axioms of section 38. The following points must also be remembered:—

(*a*) In most places the campaign can never cease—the disease can seldom be extirpated once and for ever, until civilisation has reached a much higher state. We should be prepared for the fact that malaria reduction must become a permanent part of the general sanitary campaign.

(*b*) Unless the work is to be a pretence it cannot be attempted without expense. Perhaps the ideals of the third and fourth axioms cannot be reached—but they must be remembered. Rarely can we destroy so redoubtable an enemy without some *goldshed.* But every penny spent is likely to save pounds both to Government and to the public. Moreover, though the war may never cease, the cost of it is

likely to diminish with good administration, and especially with success.

(*c*) Public instruction must always be given, but, except perhaps in very civilised states, it is not likely to be entirely effective by itself. The individual may take quinine, use nets, or prevent breeding in his premises, but he cannot force his neighbours to do so, nor can he clear the surrounding country or drain the marshes. Only the better educated classes are likely to attend to sanitary advice. The great public is an infant for which everything must be done by the nurse. The poor are too poor, the idle too idle, the busy too busy, to concern themselves much with sanitary injunctions. Nevertheless, instruction costs the state little and may occasionally reach the small percentage of the wise.

(*d*) Sanitation is a form of war. It requires not only money and effort, but also thought, organisation and discipline.

(2). *Appointment of a directory.*—This being the case the first thing to do is to appoint a commandant. For a small campaign in a town or district, the proposer of it, whether he be an administrative, sanitary or medical officer, had better appoint himself (without salary) and then proceed to collect what army he can in the form of sanitary inspectors, medical assistants and even policemen. He can then begin to ask for funds. But this is personal effort; and for larger areas Government should appoint a special officer in the Sanitary Department. His pay and status must depend upon the area to be dealt with. He should be thoroughly acquainted with the subject and also with general sanitary administration—a medical man, or possibly an entomologist or engineer, or for very large areas an administrative officer.

For entire countries I advocate the construction at once of a Malaria Bureau as part of the general Sanitary Department—

that is, a director with a trained "malariologist," an entomologist, a sanitary engineer, and a statistician as subordinates, and a suitable office. The bureau will collect information and co-ordinate efforts throughout the country, will carry out inspections, give advice, examine results, and publish reports.

Those who have no acquaintance with public affairs may think that this is an extravagant suggestion. In 1904 I mooted the idea for India through the Secretary of State. In that country malaria has been officially estimated to cause about 1,000,000 deaths annually. Government spends enormous sums on plague, which has a smaller mortality and much smaller morbidity; but a mere cipher on malaria. My suggestion was not accepted, and not even a director was appointed. Hence the efforts made there have been, in my opinion, too discontinuous for success (section 59), and much of the money spent has possibly been wasted.

The head of the Sanitary Department must often himself direct the anti-malaria work. He will do well to begin at once by collecting a permanent special staff. This will cost money; but it will save much more which is otherwise likely to be wasted on inexperienced efforts. Public work is permanently successful only if directed by officials who are permanently interested in its success. Local enthusiasts perform great services in originating campaigns, but they alone cannot maintain them for ever.

General anti-malaria work is now sure to be commenced in most civilised countries, and it is high time that governments should prepare for this by providing the necessary administrative nucleus.

(3). *Anti-malaria Leagues*, under the presidency of high officials, consisting of numbers of medical men and influential persons, with a central office and local branches, should be started at once—have been, in fact, already started in many places. They serve to interest the public, to disarm opposition, and to enlist active allies. To them should be entrusted the

dissemination of information regarding the disease by the well-known methods of placards, pamphlets, postcards and lectures. Personally, I think it wise not to demand a general subscription for membership, as this tends to limit the list, but to ask for donations from Government and wealthy individuals in order to pay for the printing and other small expenses of the League. Unfortunately, there is a distinct danger that the Government, as soon as the League has been formed, will attempt to shift responsibility on to it. It is not often that the League will be able to do the whole work. Its duty lies principally in arousing public attention as regards protective measures and quinine. It can seldom undertake extensive mosquito reduction or much quinine distribution. Lastly, as soon as the novelty wears off, the efforts of the League may tend to slacken.

(4). *Legislation.*—The next thing required is to reconsider the local Government and municipal sanitary regulations in order to take power to deal with mosquitos and other details. This is largely a lawyer's matter, as most regulations are so framed that they may be interpreted in various ways. The Government will do well, therefore, to call together a committee to consider the subject thoroughly. Certain clauses are given as examples in section 64. It is easy to overrate the importance of legislation in the campaign. The experienced health officer depends upon it as little as possible; but it is, nevertheless, occasionally useful to control objectors and other foolish people. Thus, in Egypt my brothers found it better to ignore such persons rather than waste time over compulsion, and my experience has always been the same. The trouble and expense of bringing recalcitrants to book is greater than what is incurred in doing the work by departmental agency. The wise health officer seeks to trouble the public as little as possible, and the amateur one as much as possible. Laws protecting indentured coolies should be carefully revised in the same sense, though this is seldom necessary. All this costs nothing, but requires considerable time to effect in consequence of the unavoidable and

often unnecessary discussions which arise. I apprehend that these discussions will now become shorter, because every colony is adopting the required legislation. *See* especially R. Boyce's book [1910] and sections 59 and 64.

A serious difficulty in the way of all sanitary improvement lies in the inadequacy of punishment often given by legislators for sanitary contraventions. These are too small to be deterrent. Endless petitions are allowed; and the time of the Sanitary Department, which should be spent on useful work, is used up in attending courts and writing up cases—a simple waste of public funds. This is not freedom but licence. It is a very low form of civilisation in which individuals are allowed to abuse the freedom which civilisation has given them by poisoning or infecting their neighbours. If those who make and administer the laws were called upon to sit by the side of dying patients, as medical men too often are, and to watch the effects of the bereavements that follow, they would not so often treat these serious offences as jests or as petty failings. The success of the work at Panama has been largely due to the stringent discipline exerted; and the public themselves have become thankful for it. Sanitary discipline is like that of the sick-room, against which only the most foolish patient rebels—to his own cost. I would, therefore, strongly advise the Government which determines upon a general war against malaria to tighten up the reins in this matter.

(5). *The preliminary malaria survey.*—The necessity for *repeated* measurements of malaria is quite obvious. Without them we cannot (*a*) make any estimate of the expense which might theoretically be incurred for prevention by axioms 3 and 4, and (*b*) determine the exact effect of the measures which are taken. Thus, the authorities will not feel the necessity for the demands made on them, nor the value of the work done, and the whole campaign may tend to collapse. This happened in 1899 in Sierra Leone, where the authorities disliked a malaria survey because they feared that it would alarm the public.

Hence, while the League is being formed, the Director will do well to spend some months in making his preliminary measurements according to the principles given in sections 31 and 32. Maps being obtained, the local *total* death-rates for as many years as possible should be carefully scrutinised and compared with the attendances for malaria at hospitals and dispensaries—the results being entered in the maps and registers. This will cost nothing, but will yield information regarding both local distribution and general prevalence which will be invaluable for testing the success of future measures.

At the same time the measures proposed in next sub-section to be adopted in schools, including registration of enlarged spleen, should be commenced.

It is now also advisable to make, if possible, a general "spleen census." That this can be done on a large scale at little expense has been proved in Mauritius and Ceylon, where 31,022 and 92,258 children respectively were examined (section 22). Of course, the work cannot be done entirely by the Director himself. Orders should be issued to Government medical officers to undertake it, each for his own district, and the help of all medical men, of trustworthy assistants and dispensers, and of large employers of labour should be solicited. Travelling expenses must occasionally be paid, and I think that fees ought to be allowed under certain circumstances, as the gratuitous services of medical men are too often demanded as it is. The Director should set the example by examining as many children as possible, and he must, of course, register all the results.

The season during which the spleen census is taken is important. In the middle of the malaria season the figures are probably very variable; so that it would be better to select a time before or after that season. I advise between one and two months before the commencement of the season, or a period within about one month after the end of it — in the hope of obtaining the minimum and maximum spleen indices

respectively. In fact, for the first year I advise that the census be taken twice, namely, at the periods mentioned. Of course, it is not necessary or possible that all the children should be examined on the same day, but I think that the work ought to be done within about one month.

(6). *Measures in schools.*—From this point the actual campaign commences, and the first principle requiring attention is that *the cheapest measures should first be set in train.* The Director will be able to do much at little cost with various public institutions according to the principles laid down in section 37.

Schools, if they exist, furnish an admirable opportunity for large-scale work. In most countries the schools are either maintained entirely by Government or are given what are called "grants in aid," or at least licences. This enables Government to maintain a close control over the health of the children, and the Director to use the schools for his great purpose. Two uses can be made of them, (*a*) by the treatment of children with fever or enlarged spleen, and (*b*) for the repeated measurement of malaria. Both measures are especially suitable for children since they are the principal homes of the parasites. I advise as follows:—

(1). That every school be visited once a quarter by a suitable medical officer (who may, if necessary, be paid for the work).

(2). That at this examination the medical officer shall select all children with enlarged spleen, or with fever at the time of his visit, and should enter their names in a register kept by the schoolmaster.

(3). That at the same time the medical officer shall advise the schoolmaster as to the treatment to be given to the child during the next three months.

(4). After the inspection the medical officer should report to the Malaria Director the number of children found to show signs of malaria, and the latter should incorporate the facts in his statistics.

(5). At the next inspection the medical officer should observe the number of newly-infected children and those who have become cured, and should again report the facts to the Malaria Director.

(6). The schoolmaster should give the medicine to the children as ordered by the medical inspector.

(7). Quinine may also be advised for the children who are not suffering from fever or enlargement of the spleen, but no compulsion should be used to force them to take it.

(8). The Medical Department should supply quinine in freshly-made pills or tablets, in appropriate doses, to each school. This may be done gratuitously for the schools of the poor, but I think that payment should be demanded in some cases. Different doses should be put up in different canisters, and sweets may be allowed for small children.

(9). The medical inspector may stop the drug, if in his opinion it disagrees with a child.

I do not advise here as to the amount of the drug, which I think had better be left to the medical officer. A useful plan is for the schoolmaster to give the medicine when the child first comes to school in the morning. It may not be considered advisable to dose the children every day during the whole three months, though this may perhaps be done at certain times. Perhaps the first fortnight of each month ought to suffice, at least for a trial. During the malaria season the drug may be given more continuously, and during the non-malarious season, less so.

(7). *Measures on estates, factories, etc.*—There are generally large numbers of children in connection with the above, and these may be managed in exactly the same manner as children in schools. Estates often have their own medical officers, who should be directed to keep the registers, to give the quinine, and to report every quarter to the Malaria Director. The

latter will thus possess an excellent standing record of the state of health upon each estate or factory.

In addition to this adults on estates and factories must also be continuously treated if they are sick. The defect has generally been that they are given doses only for a day or two, thus necessitating their frequent presence at the dispensary, a thing which causes endless trouble both to dispensers and to patients. It is much better to give sufficient quinine to last for a week, or fortnight, or more, according to section 36.

Whether quinine should be used for prophylactic purposes depends upon the severity of the local endemicity, the possibility of reducing Anophelines, and the judgment of the Director (see especially section 57).

Whether quinine is to be supplied gratis to plantations, etc., depends upon the local labour regulations.

(8). *Other quinine distribution.*—I suggest the following steps in accordance with section 36:—

(1) A number of *quinine dispensers*, who are duly qualified dispensers, should be appointed under the Malaria Director.

(2) Each dispenser should be stationed at a convenient centre within an area allotted to him, and should spend the whole of his time in house-to-house distribution of quinine to those who he thinks require it.

(3) He should be provided with a small portable case, to be carried by himself, containing a day's supply of quinine in the form of pills or tablets, put up in canisters of different doses.

(4) He should be provided with a uniform, or at least an official badge, and a written authority stating his duties.

(5) Every day he should visit a number of houses allotted to him.

(6) On coming to a house he should offer the quinine

gratis to all persons who suffer from fever or enlargement of the spleen. He must not demand nor take any payment whatever, either for medicine or for his advice.

(7) He may give the drug to any person who demands it, in doses sufficient for a week or a fortnight, if he thinks that that person is infected or is in imminent danger of becoming infected.

(8) He may also examine children for enlargement of the spleen, if permitted to do so by the occupiers of the houses, and shall keep a record of the number in which he finds this symptom.

(9) The quinine dispensers should be well instructed in their duties, and in a knowledge of malaria in general. They should furnish a report every month to the Malaria Director.

(10) The Malaria Director will be well advised to send these men most frequently to the most intensely infected areas, and utilise them also, if necessary, for providing vermifuges and sulphur ointment to the very poor. The cost of this will be but small and the advantage great.

In addition to the quinine dispensers, I advised in my report [1908] that a change should be made as regards the distributing of quinine to out-door patients of hospitals and dispensaries. It is quite insufficient to give patients only a few doses for a few days. Enough should be given for a fortnight or more.

In Mauritius we found that one dispenser could visit 200 houses a week, and give away about 3·3 lbs. (about 1,200 grammes) of quinine to about 650 sick, or nearly 3 grammes to each.

I must again remark that to be really useful the drug must simply be poured out in unlimited quantities. A certain amount may be sold, but stinginess in this particular tends merely to defeat the main object in view. Government quinine may

be distinguished by a certain colour, as done in India; authorities should guard against the possibility of their quinine being subsequently retailed to purchasers or even exported; but too many precautions will have the effect only of ruining the whole scheme, the object of which is wide distribution, for the sake of saving a few pounds. The cost of collecting money for small sales, and of sending in reports of exact amounts distributed, really exceeds the cost of wastage, unless this is allowed entirely without any limit. Government must, I fear, face a certain loss from leakage, but this should be looked upon as an unavoidable part of the general cost of the campaign.

(9). *Appointment and instruction of moustiquiers.*—We now turn to mosquito reduction. The first necessity, an absolute one, is for the Director to have a number of subordinates well trained in a knowledge of mosquitos. In Mauritius we called these men moustiquiers. They were paid about 25 rupees a month, and were mostly of Indian extraction. The men should be carefully selected for intelligence and reliability by the Director himself, and should be formed into a class to be trained by him personally. It will be found that they learn their work in a surprisingly short time, and generally take great interest in it. Each man should be taught how to catch adult mosquitos, and how to find larvae in all possible breeding-places; also how to fill up holes in trees and rocks, and to direct the gangs of workmen required for clearing pools and streams, etc. The appointment should be made with a view to permanency, with a scale of increasing salaries, and, if possible, with a promise of pension. This is obviously an economical principle, because after a year or so moustiquiers become highly experienced in their work. After he is trained, each man should be allotted a given district, consisting either of town or of village, or of open country, according to the judgment of the Director. The duties of the moustiquiers will consist in ascertaining all the breeding-places in his district, in guiding the works of the malaria gangs, and in making occasional

reports to the Director. Oral reports generally suffice. It may be advisable to appoint head moustiquiers to superintend larger areas. Each moustiquier should be provided with a white enamelled pan or sieve for finding larvae in water.

These men are essential for a continued campaign against mosquitos. They are, so to speak, the local eyes of the Director. Without them there will be no permanence in the measures, because directors and superintending medical officers may change from time to time, while the staff of moustiquiers will be permanent. In fact, many campaigns have failed because this fundamental point was overlooked.

The number of moustiquiers will depend upon the amount of work allotted to each. I suppose that one man can superintend quite 10 square miles of area, except in crowded towns. If the campaign is to include Culicine reduction, more men will be required for towns.

Moustiquiers are exposed to considerable risk of infection. Care must be taken of their health, and they must be provided with bed-nets.

The Director can send his most reliable men to outlying stations and plantations in order to report to him as to mosquito breeding there, and also in order to instruct managers, headmen of villages, and so on—a thing which he cannot always do himself.

(10). *Malaria gangs for minor works.*—The moustiquiers do not do manual labour, which is entrusted to workmen of the class usually employed for gardening and engineering works. The duty of these workmen should be to train small pools, to keep street gutters, surface drains, roadside ditches and channels, margins of ponds and streams clear of weeds and obstructions; to carry out rough canalisation of streams, to deepen or "back-up" breeding-ponds; to fill or oil other waters; to fill holes in rocks and holes in trees; to cut undergrowth; and to deal with house waters which breed Culicines

From our experience in Mauritius I advised that the workmen be formed into gangs of about three or four men each. One of the men should be appointed the headman of each gang, with extra pay, and should be responsible for the work of the gang. This extra pay is an inducement for the workmen to remain in such employment, which, like that of moustiquiers, requires considerable training for economical working; and it is astonishing how much trained men will do in a given time.

It is impossible to state how many gangs will be required for a given area. This will depend entirely upon the local conditions. In Mauritius I thought that one gang could manage about 4 square miles as a very rough average, and the cost of each gang was about 50 rupees (£3·3) a month.

The gangs are to be superintended by the moustiquiers, or by special headmen if necessary; but the Director must frequently examine the work done. It may also be frequently feasible to put the gangs under enthusiastic local medical officers, or district officers, or members of the Anti-malaria League, subject, of course, to the final authority of the Director.

(11). *Local offices.* — All this organisation — dispensers, moustiquiers and malaria gangs — will require a number of local offices where the men can be given instructions, paid, supplied with medicines, oil, tools and impliments, etc. Generally, as the whole organisation is under the Medical Department, it will be better to make local dispensaries the headquarters of each local malaria administration; but, of course, this question depends upon conditions. I do not think it is necessary to appoint a special clerk at each dispensary. Perhaps the sanitary inspector's office may be more convenient than the dispensary in many cases.

(12). *Major works.* — Here the fundamental principle of 35 (8), namely, that of *minor works before major works*, must always be remembered. It is folly to commit the state to a

large expenditure where a small one may suffice. At the same time it may sometimes be more economical to do the major work at once. Hence, at an early stage of the campaign, the Director should obtain rough conjectural estimates of the cost of all major works that may be required. *Exact* estimates can only be obtained from the Public Works' Department at considerable cost, incurred for accurate surveying and skilled advice; but Government will generally be able to ask its engineers to prepare the *rough* estimates by mere inspection of the localities concerned. These rough estimates are now borne in mind by the Malaria Director, who shapes his policy accordingly.

Of course, major works will generally demand not only a considerable capital outlay, but also an annual expenditure for maintenance. Government always finds a considerable difficulty in obtaining the former item, which may require loans and the consent of higher authorities. The Director should, therefore, generally not make such demands until he is convinced by the unsuccessful result of minor works during several years that the expenditure is absolutely necessary; unless he and the Public Works' Department consider that money will thereby be saved in the end.

The important question as to who is to pay for drainage of marshes on private property often arises; and I was asked in Mauritius to advise upon the point. I first enquired officially whether owners could be forced by law to drain such marshes, or to pay for the drainage of them by Government, especially in view of the fact that such drainage might greatly enhance the value of their property. In reply I was informed that in the opinion of the Law Officer, marshes could not be held to be nuisances in the statutory sense of the word; and that legislation had been proposed but presented many difficulties. The answer seems to me to depend upon (*a*) whether the existence of the marsh is or is not the fault of the owner, and (*b*) whether the removal of it would or would not benefit

the general public as well as himself. For instance, if there is in existence a sufficient and practical outfall for the marsh, or other means of dealing with it in the owner's property, but one which the owner neglects to use, or refuses for his own profit to use; and if he neglects to render the marsh innocuous by such reasonable minor works as may be required by the Medical Department; and if it is proved that the marsh is actually causing sickness, or, in the opinion of the Medical Department, is likely to cause sickness; then I think that he (the owner) may be forced to do the work. As a matter of fact a marsh in a malarious country is a nuisance, and the fact that this is not recognised by law proves only the inefficiency of the latter. If a person can be forced to remove or clean a latrine, he ought most certainly be forced to remove or discharge a marsh, and for the same reason.

On the other hand, if the owner of a marsh has done all reasonable minor works to render it innocuous; if the major work is beyond his means, or cannot be carried out on his property; and if the marsh is not causing or is not immediately likely to cause public sickness, then I doubt whether he can fairly be forced to undertake the expense. Each case must be judged on its merits; and Government will often feel it more fair to pay at least a part of the expense. At the same time, legislation to compel defaulting owners should certainly be passed.

Precisely similar difficulties often occur in connection with water-ways, drains, polluted rivers, and especially irrigation channels; and I think they may generally be met on the same principle.

(13). *The annual malaria report.*—I think it is most essential that a report upon the work done should be submitted to Government every year. It should be prepared by the Malaria Director, and should commence with a detailed statement of the measurement of malaria for previous years as well as for the year under review. It should then describe the measures

taken, the number of men employed, and the cost; and should conclude with remarks upon the results obtained.

This report will enable Government to judge whether or not value is being received for the money spent. Officials are apt to dislike the trouble incurred, but it is a necessary one. In British possessions, one often finds a single perfunctory annual report on medical matters, and we should compare such with those excellent monthly reports issued by the Americans for the Panama Canal Zone.

(14). *Cost.*—I will content myself by giving the approximate estimate which I prepared for Mauritius [1908].

Items	Rs. per annum.
1. Salary of the Malaria Authority	6,000
2. Salaries of 5 Quinine Dispensers (say)	6,000
3. Salaries of 15 Moustiquiers	4,560
4. Salaries of 109 gangs (327 men)	65,400
5. Cost of quinine (say)	30,000
6. Preparation and despatch of quinine (say)	3,600
7. Office of Malaria Authority (say)	1,500
8. Implements, etc., for gangs (say Rs. 50 a gang)	5,450
9. Travelling expenses for staff (say)	2,000
10. Margin for possible calls	10,490
TOTAL	135,000

This amounts to £9,000 per annum for a population of 375,400 (1906), or 0·36 rupees per head per annum, and 1·2% of the total revenue.

The estimate was an "outside one." The number of gangs of men was determined, not by myself, but by the Medical Department, and many suppose that it is considerably in excess of the actual requirements. In fact, I have since heard that the whole work can probably be done at much less cost. My whole scheme has, however, been held over pending the final settlement of the financial position of the Colony. This has only just been effected (1910), and I hear that the scheme will now be proceeded with.

In addition to the above annual expenditure, we had to

consider the major works. A rough conjectural estimate for these was prepared by the Public Works Department, and was put at 630,000 rupees for capital expenditure, together with 42,300 rupees for annual maintenance. But, as I have said, I hope that experience will prove that little of this will really be called up.

Critics of my report have complained that such expenditure, though it may be possible for Mauritius, will be altogether too vast for other countries. The total revenue of Mauritius is 9,915,863 rupees, of which 749,827 was allotted to the Medical and Health Department and to quarantine. Hence the proposed malaria expenditure amounted only to 1·2% of the total revenue, and to 18% of the medical and sanitary revenue; while the disease causes 1/3 of the total sickness, and costs the colony about 1,000,000 rupees a year (section 32). The sum of about one-third of a rupee per head of population is scarcely too much to pay for all the advantages, not only of malaria reduction, but of very considerable mosquito reduction, and general "cleaning up."

Moreover, I may note again that the malaria gangs can be largely utilised for general sanitary purposes, and for performing many useful offices during the non-malarial season. On the whole I am quite convinced that this expenditure would be a very small one for any population, compared with the benefits obtained.

Of course, the Malaria Director will have to prepare similar estimates before he engages large numbers of men. Unfortunately, it is always impossible to state without trial how many gangs are required for a given area. Thus, the Director is almost forced to commence with the most populous areas, and especially with the most malarious ones. He must point out these difficulties to the Government, and must do the best he can.

(15). *Remarks.*—Amateurs are fond of advising that all practical measures should be postponed pending the carrying

out of detailed researches upon the habits of the Anophelines, the parasite rate of localities, the effect of minor works, and so on. In my opinion this is a fundamental mistake. It implies the sacrifice of life and health on a large scale, while researches which may have little real value, and which may be continued indefinitely are being attempted. As a matter of fact, the campaigns at Havana, Panama, Ismailia, and the Federated Malay States, were all commenced before the local carriers were definitely incriminated and their habits studied. We already know the broad general principles that these carriers breed in more or less stagnant waters, and that protection and prevention by treatment are valuable. More exact knowledge of the habits of the carriers is certainly often very valuable, because it enables us to deal more economically with them; but, if the principle of minor works before major works is remembered, we need not wait for the full development of such studies. Moreover, the Medical Director will be able to carry out such researches much more effectively by the aid of his moustiquiers and quinine dispensers than by himself in a long preliminary survey during which the people are allowed to continue dying. In practical life we observe that the best practical discoveries are obtained during the execution of practical work, and that long academical discussions are apt to lead to nothing but academical profit. Action and investigation together do more than either of these alone.

I strongly advocate a generalised campaign in preference to one which deals only with a few spots. The former is likely to be more economical in the end, and the latter is more likely to dishearten the authorities and to waste money. At the same time financial considerations will almost always demand stronger efforts in the larger, more populous, and most malarious localities — at least to begin with. But the Director must always hold in mind and prepare for extension to every part of his domain.

In conclusion, I believe that anti-malaria work can be

effected for little cost in all malarious countries, and will soon be attempted everywhere. The success of it depends, not so much on profuse expenditure, as on the intelligence, enthusiasm and energy of those who are responsible for sanitary affairs. He who undertakes the work must remember that he is a soldier in one of the greatest of crusades, which has for its object the conquest of the tropics for humanity.

CHAPTER VII

SPECIAL CONTRIBUTIONS

By Professor L. O. HOWARD

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41. Anti-malaria Work in the United States.—Beginning with 1900 and gradually increasing in amount year by year, a great deal of anti-mosquito work, more or less well planned, has been done in different parts of the United States; but, looking over the entire field, it is astonishing to note how little of this work has been done with a direct sanitary object in view. The sanitary point has been held constantly in mind in the operations conducted under the United States Government in Cuba, on the Isthmus of Panama, in Porto Rico, and in the Philippines, but in the United States, while malaria is prevalent in large regions, such work as has been done has been instituted for the most part with the sole idea of relieving localities from the nuisance of mosquitos, thus not only rendering living conditions more agreeable, but increasing the value of real estate. In fact, one of the best bits of work done was accomplished by means of a large sum of money presented by a wealthy man whose object was not primarily the improved health of the people of the region, but bettering the condition of his high-priced race-horses, which were suffering from the abundance of mosquitos. Nevertheless, what anti-mosquito work has been done naturally has resulted in improved conditions in health in localities included within the scope of operations.

A great deal of valuable drainage work has been done in the past few years in the salt marsh region of the North

Atlantic coast, and there is one instance of this upon the Pacific coast, with the direct idea of doing away with the salt marsh mosquitos, several species of which occur in such localities, all having unusual power of flight, and being able to proceed inland for many miles, thus annoying the inhabitants of a large extent of country. In America *Anopheles* mosquitos are rarely found in these salt marshes, and, in fact, there is but one record of the finding of *Anopheles* larvae in such locations in the United States.

One of the first operations of this kind was conducted by the wealthy owners of Center Island, off the north coast of Long Island in Long Island Sound. This led to somewhat elaborate work under the organisation known as the North Shore Improvement Association, which included simple operations over a considerable distance along the north shore of Long Island, and in the vicinity of Oyster Bay. In the course of this work the breeding-places of *Anopheles* were studied and mapped, and especial effort was made to destroy them. These operations took place in 1902 and 1903, and have been reported at length in a small volume published by the Association. Later, some excellent work was done at Lawrence, Long Island, which demonstrated the feasibility of controlling the salt marsh mosquitos by relatively simple and comparatively inexpensive ditching operations. These operations were continued for four years at an annual expense of something more than one thousand dollars per year.

The work in the early part of this short period of years was usually started here and there by intelligent and up-to-date citizens, citizens' associations, city improvement societies, and women's clubs. The California work, taken up in 1905, was done under the auspices of the Burlingame Improvement Club. Excellent anti-mosquito work was begun as early as 1900 on Staten Island, New York, by the Richmond County Club. Admirable community work was taken up during 1901 and 1902 by certain New Jersey towns, notably South Orange,

Elizabeth, Montclair, Monmouth Beach and Summit, and a little later independent work was begun in Greater New York under Dr Lederle, which included the mapping of all mosquito breeding-places within the city limits. Independently, the health officers of Brooklyn, Jamaica and the Brinx began efficient work, while the summer resorts of Arverne and Woodmere reduced the mosquito supply by intelligent operations. At Willets Point, New York, intelligent and efficient work was carried on on a small scale. In Massachusetts interesting and important work was done at Brooklyn and at Worcester.

In Brooklyn the Board of Health first considered the work in August 1901, and in September all the breeding-places of *Anopheles* and other mosquitos were treated. In 1902 all pools, ponds, ditches and other breeding-places, including catch-basins, were located on the town map, the approximate areas were determined, and the number of catch-basins ascertained; breeding-places of *Culex* and *Anopheles* respectively were determined, and also places where both species were breeding together—this being done in order to ascertain the proper intervals for treatment—that is, whether every two weeks or every three weeks. Public dumps and other places where accidental receptacles of water might be found were located on the maps. Light fuel oil was used on all breeding-places.

At Worcester the work was of the most interesting kind. Dr William M'Kibben and Dr C. F. Hodge started the crusade. Breeding-places were mapped and photographed, and public lectures were given. The school children of the several grades were interested, and were organised into searching parties. Many breeding-places were filled up, and others were treated with kerosene. The prevalence of malaria in Worcester was pointed out by those engaged in the crusade, and the relation between mosquito breeding-places and the houses where malaria patients lived was shown. A map was prepared showing the exact distribution of malaria in the city, and photographs

were made showing the character of the breeding-places of the malarial mosquito. It is probable that these Worcester efforts to interest the school children were the first made in this direction, although the idea was carried out to a much greater extent later in San Antonio, Texas, under Dr J. S. Lankford.

Other early work was carried on at Pine Orchard and Ansonia, Connecticut, and Old Orchard Beach in Maine, and on the campus of the Michigan Agricultural College in Michigan. At Atlanta, Georgia, the Sanitary Department used a large amount of kerosene in the stagnant pools and the swampy places around the city, and warned the citizens to watch their rain barrels and to keep their gutters open. A great many pools of water were drained. In Savannah some work was done, and the number of mosquitos was reduced very considerably. At Talladega, Alabama, under the direction of Dr B. B. Simms, anti-mosquito work was commenced early in 1902, and carried out systematically. St Louis took up the work early in 1903, and the Municipal Assembly made an appropriation for supplies. The Health Department, however, was hampered for lack of men and little work was done.

Many of these and other efforts were spasmodic and only temporary in their effect; even the boards of health in some instances lost interest. An excellent example of the difficulties encountered by intelligent citizens was shown in the city of Baltimore. Intelligent individuals pointed out very early that a large part of the mosquito supply of the city could be easily handled, and year after year, in public press and before the Board of Health and the City Council, these persons continually agitated the subject of anti-mosquito and anti-malarial work. Finally, in 1907, Mr George Stewart Brown, a member of the City Council, succeeded in getting an appropriation to start the work for that year. Much of this money was expended in extensive advertising in the street cars, etc., but the remainder was expended very efficiently, though necessarily

with only partial results, by organising a gang of men to drain and fill up the pools in the vacant lots around the suburbs. Next year the appropriation was reduced, and only this gang of men was continued. During 1909 no appropriation was made, the gang of men was dropped, and the whole question was abandoned. It should be stated, however, that before the appropriation was made an ordinance was passed by the City Council requiring every householder to remove, screen with wire-netting, or keep covered with oil all standing water around his premises, but it seems that no real attempt was ever made to enforce this ordinance. Of course such an attempt could hardly be successful at first without the aid of an especial appropriation for the purpose, and at the present time the ordinance seems to be a dead letter.

It is pleasing, in the face of so much of this sort of thing, to note a well-directed and rather large-scale bit of work with a direct anti-malarial bearing which was begun at an early date, and that is the work on Staten Island under Dr A. H. Doty, the health officer of the port of New York.

Staten Island, lying in New York Harbour, had had a rather unenviable reputation on account of the great number of mosquitos present and the continued presence of malaria. It was largely on account of the latter condition that Dr Doty began his investigation in 1901. He soon found that there were two factors to deal with in this work, namely, the inland mosquitos and the salt marsh mosquitos.

In the extermination of the inland mosquitos, the section of Staten Island which was known to contain many cases of malaria both in the acute and chronic forms was selected for experimental work. This section consisted of a basin or low-land about a mile square, containing about one hundred small dwelling-houses some distance apart. Within its boundaries were a large number of stagnant pools varying in size from ten feet in diameter to an acre or more in area. A house-to-house visit showed that at least 20% of the inhabitants of this

district were suffering with some form of malaria, and in the immediate vicinity of every house were found typical breeding-places in the shape of old tinware, rain-water barrels, cisterns, cesspools and ground depressions, many of which contained larvae. For the purpose of detecting the presence of adult *Anopheles*, glass tubes fitted with cotton plugs were distributed among the occupants of these houses with the request that the mosquitos found in the house at night be captured and placed in the tubes. In the collection were found many *Anopheles*. These were particularly numerous in tubes coming from a small group of houses. In one of the latter was found a family consisting of five persons, all of whom showed the acute or chronic form of malaria. Dr Doty himself secured live mosquitos from the interior of this house. On the first evening five were captured, and all but one were *Anopheles*. On the second evening twenty-two were collected, and of these more than one half were *Anopheles*. In the house on the opposite corner was found a patient suffering from an acute attack.

In the beginning considerable difficulty was found in detecting the breeding-places of the *Anopheles*, but this became easier as the inspections became more thorough. For instance, in a group of two or three houses close together a number of *Anopheles* were captured, but their breeding-place could not be found for some time. Finally, in the backyard of one of the houses, overgrown with weeds, was discovered a very large metal receptacle filled with *Anopheles* larvae, and with many adults in the immediate vicinity. This receptacle was almost entirely covered by underbrush.

After this experience the men employed learned to make the closest possible search, and to find probably every breeding-place.

The island was then divided into small districts, which were visited by mosquito corps consisting of five men, one of whom was a sanitary police officer connected with the New York City Department of Health. The equipment of the mosquito

corps consisted of a large waggon provided with spades, rakes, hoes, scythes, and petroleum oil. A house-to-house inspection was made in each district. House owners or tenants were required to remove from about the premises all receptacles which might act as breeding-places, or to protect them. Rain-water barrels and cisterns were covered with wire-netting, all roof gutters were repaired, and pools of water were covered with petroleum. In certain instances orders were sent to the owners of property containing depressions in the soil to fill them in or drain them. If these orders could not be enforced, the mosquito corps returned every ten days or two weeks and applied more petroleum. Copies of a circular of information were delivered so far as possible to each house on Staten Island by police officers, and this educational campaign brought about valuable co-operation on the part of the public.

In 1905 the details of this work were presented to the Department of Health of the city of New York, and the city government granted an appropriation for the drainage of the swamp land along the entire coast of the island. With the aid of this appropriation, ditching was carried on somewhat in the same manner in which it has been carried on in New Jersey. Down to the present time between 800 and 1,000 miles of ditches have been dug. The swarms of mosquitos soon practically disappeared, window screens were discarded, and meals were served upon the verandas of the hotels.

With the malarial and other inland mosquitos the work was carried on in the manner above described, not only in the built-up portion of the island, but also in the open spaces between the small and scattered settlements. During the past two years cases of malaria on Staten Island are becoming practically unknown, and for the past year Dr Doty has been unable to secure any *Anopheles*, whereas, in the beginning of the investigation they were found almost everywhere on the island. The statistics of the Department of Health indicate the decrease of malaria from 1905 on. Prior to 1905 malaria

was not regularly reported, but the number of cases was surely very much greater than that reported in that year. Since 1905, however, they are stated to be as follows:—1905, thirty-three cases; 1906, fifty-four cases; 1907, four cases; 1908, six cases; 1909, five cases.

The work of exterminating malarial mosquitos has been necessarily slow, as the area involved is considerable, the island being about 16 miles long and 4 to 6 miles wide, probably containing over eighty thousand inhabitants in large areas between the various towns.

The expense of the operations down to the present date has been about $50,000; this, of course, includes the expense of the extensive drainage operations in the salt marshes. Dr Doty, in addition to being the Health Officer of the Port of New York, is a Commissioner of Health of New York City, and he carried out this work in his capacity as a municipal officer, and not as a State official.

Earlier and very much smaller pieces of work have previously been described by the writer in "Mosquitos" (New York, 1901), including the work done by Dr W. N. Berkeley in a small town near New York City during the summer of 1900, and the work done at Chaptico, Maryland, by the Rev. William Brayshaw, in both of these cases the work being directed primarily against malaria. Later, some excellent work was instituted through the combined action of the boards of health of Cambridge and Belmont, Massachusetts, to improve the sanitary conditions of the cities of Cambridge and Somerville, and the towns of Arlington and Belmont, at the inspiration of Mr W. L. Underwood, a member of one of the boards of health. This was effectively carried out at an expense of $600 without assessment upon land-holders.

In 1903 some extensive work was done at Newport, Rhode Island, at the expense of a single property holder, and the following year the Citizens' Association of Flushing, Long Island, took up the problem, and with the assistance of the

Board of Health extensive drainage operations have been carried on, but are not yet completed.

In 1903 the American Mosquito Extermination Society was founded, and took an active part in interesting the people of the United States in the mosquito crusade. It was founded for the purpose of educating the public, of bringing about legislation, and of securing co-operation and exchange of ideas. It held its first anti-mosquito convention 16th December 1903, and continued its organisation, publishing several bulletins, finally disbanding in 1907 in consideration of the fact that the objects of its existence would be taken over by the National Drainage Association.

In 1903 and 1904 work against mosquitos was undertaken by the state entomologist of Connecticut, Dr W. E. Britton, who made careful mosquito surveys over the whole state, and he published in his Annual Report for 1904 a careful and well-illustrated article devoted to showing how the mosquito nuisance can be abated.

The largest-scale work taken up in the United States, however, has been carried on by the state of New Jersey, and this is the first of the United States to take official action in this direction. This work was instigated by Dr John B. Smith, the state entomologist, under whose direction it has been carried on, and resulted largely from his discovery of the exact habits of the salt marsh mosquitos. An admirable state law was passed carrying a very considerable appropriation. The law provided for a survey at the request of the board of health of any city, town or township, which should map all mosquito breeding-places, and provided that the board of health must, after the survey, proceed to abolish the breeding-places. The law provides further that if the necessary cost of the work shall equal or exceed the value of the land without increasing its taxable value the board may apply to the director of the work, who may, if he deems the matter of sufficient public interest, contribute from the state funds to the cost of the necessary

work, providing that not more than 50% of the amount shall be contributed in any case. Under this appropriation and during the past three years much admirable work has been done upon the salt marshes of the New Jersey coast. In all 25,192 acres of marsh land have been reclaimed by 3,633,974 feet of ditching, and this includes a shore line of nearly 70 miles. Other work has been done also, more specifically against inland mosquitos, including *Anopheles.*

The work that was done in New Orleans during the yellow fever outbreak of 1905 was an object lesson to the people of the United States. This work was so thorough that it resulted in the reduction of the numbers of the yellow fever mosquito to a minimum, and at the same time sensibly decreased the numbers of *Anopheles.* This work, while carried on originally under the United States Public Health and Marine Hospital Service, has been continued by the city Board of Health, and the example of New Orleans in this direction has been slowly followed by other southern cities.

The city of San Antonio, Texas, has the honour of instituting and carrying forward a large-scale effort to educate the children in the public schools, which is worthy of rather specific mention on account of its widespread application.

In November 1903 there were a few cases of yellow fever in San Antonio which caused several deaths, and an inexcusable interruption of commerce that cost hundreds of thousands of dollars. In the effort to allay the panic, the existence of yellow fever was denied, not only by persons having business interests in the city, but by many medical men as well. Very many adults not only denied the existence of the fever in the city, but denied the relation between the mosquitos and the fever. Perhaps the majority of the adults seemed too old to learn; and to the enlightened physicians it appeared that it was impossible to begin education at the wrong end of life.

The Chairman of the Sanitary Committee of the School Board (Dr J. S. Lankford) grasped the happy idea that if the

children were properly educated, sanitary matters in the future would be much better attended to. He suggested to the Board that it would be valuable to educate all of the school children of the city in prophylaxis and make sanitarians out of them all. The School Board heartily approved of the proposition, and the campaign was at once begun to educate the children on the subject of *Insects as Disease Carriers*. The best recent medical literature on the subject was procured and furnished to the teachers, and a circular letter was sent to them outlining a proposed course and offering a cash prize for the best model lesson on the subject. Teachers became deeply interested in the subject. A crude aquarium, with eggs and wrigglers, was kept in every schoolroom, where the pupils could watch them develop; and large magnifying glasses were furnished in order that they might study to better advantage. The children were encouraged to make drawings on the blackboard of mosquitos in all stages of development; lessons were given and compositions were written on the subject. Competitive examinations were held, and groups of boys and girls were sent out with the teachers on searching expeditions to find the breeding-places. Rivalry sprung up between the 10,000 public school children of the city in the matter of finding and reporting to the Health Office the greatest number of breeding-places found and breeding-places destroyed. Record was kept on the blackboards in the schools for information as to the progress of the competition, and great enthusiasm was stirred up. In addition to these measures, a course of stereopticon lectures was arranged, grouping the pupils in audiences of about one thousand from the high school down, and, in Dr Lankford's words, " It was an inspiring sight to watch these audiences of a thousand children, thoughtful, still as death, and staring with wide-open eyes at the wonders revealed by a microscope. It seemed to me that in bringing this great question of preventive medicine before public school children we had hit upon a power for good that could scarcely be over-estimated." The result of

this work, it is pleasing to say, was a decided diminution in the number of mosquitos in San Antonio. There was some opposition among the people, but the movement on the whole was very popular. One result of this work was that while there had previously been from fifty to sixty deaths a year from malarial trouble, the mortality was reduced 75% the first year after this work was begun, and in the second year *it was entirely eliminated from the mortality records of San Antonio.*

In organising community work against mosquitos, the school children hereafter must be counted upon as most important factors. Almost every child is a born naturalist, and interest in such things comes to them more readily than anything else outside of the necessities of life. They are quick-witted, wonderfully quick-sighted, and as finders-out of breeding-places they cannot be approached except by adults of the most especial training. One of the first steps that a community should take is, therefore, the encouragement of the interest of the children in the public schools.

I fear that it will appear from what precedes that, considering the economic loss existing in the United States through malaria, which has been estimated by the writer at surely not less than $100,000,000 per year (see Bulletin No. 78, Bureau of Entomology, United States Department of Agriculture), nothing like the competent work has been done that should be done, or really that should have been done in the past eight years, within the territorial limits of the United States themselves. While very many sections of the country that were originally extremely malarious have been practically freed from malaria by the drainage of swamp areas, undertaken not as a measure of sanitation but in order to add to the supply of arable land, it is nevertheless seen that the conditions of improved civilisation have brought about steadily increased opportunities for *Anopheles* to breed, and this increase in the number of breeding-places and kinds of breeding-places is constantly growing. The introduction of irrigation into many

portions of the western country has resulted in the introduction of malaria, and these operations are going on with a very rapid increment. Moreover, in regions which are not dry and which need no irrigation, the construction of mill-dams, the excavation of stone quarries, the building of railroad embankments and many other operations which follow rapidly increasing populations have resulted in the multiplication of *Anopheles*, and in many instances the introduction of malaria has been followed by its rapid spread through extended regions. On the whole, under such conditions it would not be surprising if malaria were increasing rather than decreasing in the United States, and the mosquito crusade is bound to become an extremely important matter within the next few years.

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BY COLONEL W. C. GORGAS, UNITED STATES ARMY

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42. Malaria Prevention on the Isthmus of Panama.— The Panama Canal extends diagonally across the Isthmus of Panama from south-east to north-west, a distance of 42 miles from shore to shore.

Commencing on the South Sea at Panama it runs up the valley of the Rio Grande river to Pedro Miguel, then across the divide to Bas Obispo, and from there down the valley of the Chagres to Colon. Roughly, about two-thirds of this distance is broken and mountainous, and one-third low and swampy. About one-fourth of the population lives in the lowlands and three-fourths in the highlands.

Before coming to Panama in 1904 the Army Medical Corps had had very little experience in dealing with malaria on a large scale in rural districts. The mosquito work established by the Army medical officers in the city of Havana in 1901 had not only gotten rid of yellow fever but also of malaria. This work has now been going on in Havana for the past eight years, part of the time under Army medical officers, and part of the time under Cuban officers, and is the most successful example of municipal malarial work that I know of.

The following table shows the decrease of malaria under these measures, and at the present time, its practical extinction:—

1890	1891	1892	1893	1894	1895	1896	1897	1898	1899
170	203	202	240	201	207	450	811	910	909

Mosquito work commenced February of this year, 1901.

1900	1901	1902	1903	1904	1905	1906	1907	1908	1909 (to July)
325	151	87	51	44	32	26	23	19	2

Ronald Ross's work at Ismailia was the only example at this time that I knew of, pertaining to successful anti-malaria work in rural districts.

At Panama our anti-malarial work is principally rural, located for 47 miles along the line of the railroad between Panama and Colon. The railroad in general follows the line of the canal. In this distance we have a population of about eighty thousand living within a half a mile of the railroad, and occupying some thirty villages and camps, and more or less isolated houses scattered between.

In this article I shall describe the sanitary organisation as it existed in July 1908.

This strip of 47 miles along the railroad is divided into eighteen districts, each district in charge of an inspector. The inspector's duties consist in the carrying out of the sanitary work of the district, and for this purpose he had at his disposal, on an average, about fifty men. The anti-malarial work consists in the order of its importance, of:—

(1). Drainage.
(2). Brush and grass cutting.
(3). Oiling.
(4). Use of larvicide.
(5). Prophylactic quinine.
(6). Screening.
(7). Killing mosquitos in quarters.

Drainage.—He is required to drain and do away with all pools within, approximately, 200 yards of all villages, and 100 yards of all individual houses. For this purpose we consider subsoil-drainage by far the most effective and economical; second, open concrete ditches; and, third, open ditches.

The porous subsoil pipes give a perfect anti-malarial drainage. Besides doing away with all breeding-places, they enable you to use a horse-mower on the ground so drained, and thus much cheapen the cost of subsequent grass cutting.

We use the open concrete ditch in localities where

subsoil drainage is not practicable, such as flat places where the fall is not sufficient, and small natural rills where the volume of water is too large for subsoil drainage, etc.

The objection to this style of ditch as compared with the subsoil is that it requires supervision for the purpose of keeping it free. Any obstacle in the ditch will make a small collection of water, which in this warm climate will breed mosquitos at any time of the year.

Open ditches we only use when the occupancy is going to be temporary, not more than two years. While the first cost is smaller than in either of the other class of ditches, the ultimate cost is very large. In a locality such as Panama, vegetation grows so rapidly that the ditches have to be cleared out at least once a month, and when clean they will breed larvae unless treated with larvicide.

We pay our inspectors from one hundred and twenty-five dollars per month to one hundred and seventy-five, and thus get, in general, an educated intelligent class of men. We have attached to the department an inspector of sufficient engineering education to lay out the ditches, but the local inspector is entirely responsible for their execution. In general, it is the character of work such as the farmer does through our country, and any man of ordinary intelligence is capable of carrying it out. But for its anti-malarial success it is important that the man in charge should have a good knowledge of the breeding habits of the species of mosquitos in his neighbourhood.

Brush and grass cutting.—The inspector is required to keep the tropical undergrowth cut off within 200 yards of villages and 100 yards of isolated houses. Within this area the grass must be kept less than a foot high. I consider this scarcely less important than drainage. Brush and grass shelter the adult mosquito, and they will reach a habitation from distant breeding-places by short flight, if they have the continuous protection of brush, whereas the *Anopheles* will not cross a cleared area of 100 yards.

Oiling.—We use oil where drainage is either impracticable or too costly, such as the edges of swamps, edges of streams, etc. In a large construction work such as this, temporary pools are constantly being formed. The oil should be ot such consistency that it will spread readily, but not so thin that it will evaporate too rapidly. For this purpose we use crude oil thinned with larvicide.

Larvicide.—There are many places, such as the grassy edge of ponds, or of streams, in which the *Anopheles* larvae love to breed, and where oil will not spread. In such places we use a poison which mixes in the water and which we denominate larvicide. We have done a good deal of experimenting to get an effective and, at the same time, cheap larvicide. Mr Le Prince gives the formula in his paper. We use per year about 4,500 barrels of oil and about 1,900 barrels of larvicide.

Prophylactic quinine.—We consider quinine taken for prophylactic purposes an exceedingly important measure against malaria. Quinine is given gratis to any one applying to any one of our dispensaries. We have at various points along the line 21 dispensaries. It is placed on all hotel and mess tables. But we place most reliance on our quinine dispenser. In each district we have a man attached to the dispensary who is known as the quinine dispenser. He spends the day visiting the various squads of negroes at work. He offers everybody in the squad quinine. He gives it to them as they prefer, either in pill or solution. We do not attempt to use compulsion in the matter. Last year we used 3,200 pounds of quinine. Two-thirds of this was given to employees. On an average about half of our force gets a prophylactic dose of quinine each day.

Screening.—We insist upon all Government buildings being so screened as to keep them mosquito-proof. The details of screening are very important. It should always be under the supervision of a sanitarian who understands these details. As the sanitary work progresses, screening becomes less and less

necessary. In the older stations, such as Ancon or Culebra, I doubt if screening is now necessary. The country has been so thoroughly drained that there are very few mosquitos, though we still insist upon the screening just as carefully as we did when mosquitos were bad.

Killing mosquitos in quarters. — When a locality has been recently occupied, and the general sanitary work has not had time to effect malaria, or when malaria is bad from other causes, we attempt to kill the infected mosquitos. As daylight comes on, the *Anopheles*, when she has had her fill of blood, will not in general leave the building where she has fed, but will seek some dark corner in which to spend the day. Taking advantage of this we have a man go around and kill all the mosquitos he can find in the building. In this way most of the infected mosquitos are killed before they become infectious. This method is particularly efficacious in tents and small buildings. It is surprising what results can be obtained by this method in properly selected cases.

Cost.—The total cost of sanitation on the Isthmus [1] for the past five years has averaged annually about three dollars and a half *per capita* for the whole population on the strip. During that time this population has averaged 100,000. As the general result of this expenditure our death-rate for the total population on the Zone has fallen from a maximum of 67·72 per thousand in July 1906 to 21·52 in December 1909. The rate for 1905 was 49·94; 1906—48·37; 1907—33·63; 1908—24·83; 1909—18·19. The death-rate among employees for disease has fallen from a maximum of 62·15 in July 1906 to 10·13 in December 1909.

Of the yearly expenditure of \$3·50 *per capita*, I estimate that \$2·00 *per capita* is spent on mosquito work. This expenditure has accomplished the total eradication of yellow fever. The last case on the Isthmus occurred in Colon in May 1906. It has reduced the deaths from malaria in the total

[1] Wages are high in Panama.—R. ROSS.

population from a maximum of 16·21 per thousand in July 1906 to 2·58 per thousand in December 1909.

Among employees it has reduced the deaths from malaria from a maximum of 11·59 per thousand in November 1906, to 1·23 per thousand in December 1909. The admission rate among employees has fallen from a maximum of 1263 per thousand in July 1906 to 191 per thousand in December 1909. The admission rate per thousand for malaria among employees has been as follows: 1904, 125; 1905, 514; 1906, 821; 1907, 424; 1908, 282; 1909, 215.

Wherever, in this article, I have used the expression so many per thousand for a month, I mean on the basis of a year. If I say the death-rate was ten per thousand for July, 1909, I mean that if the number of deaths and population had continued the same for the year that they were for July the rate would be ten per thousand per year.

I think that the mosquito work already done at Ismailia, Havana, Panama, and other places has demonstrated that malaria can be controlled and eventually banished from even the worst places in the tropics, and this at no very great expense. I think that the individual farmer can go anywhere into tropical countries and be free from malaria if he will drain the land and clear brush within a hundred yards about his house. And in addition screen his house so carefully that mosquitos cannot gain access. This will not cost him anything like as much as he expends in protecting himself against the cold of Dakota or Manitoba.

NOTES BY R. ROSS.—Dr H. R. Carter, the distinguished discoverer of "extrinsic incubation" in yellow fever, and until recently Director of Hospitals in the Panama Canal Zone, writes me a letter from which I am permitted to quote the following:—" Independently of the general results as shown in the general statistics, there have been some — a number — of beautiful examples of the results of anti-*Anopheles* work: *Caballo Viego*, falling from an admission rate of 16·75% per week to 3% within two months; *Porto Bello* from 10-12% to 3% and finally to 1·5%; *Juan Grande* the same; *La Boca* exceedingly bad in 1905, and in 1907 the least malarial station in the Isthmus; *Carroval*, bad in 1905 and considered almost uninhabitable

in French times, one of the most healthful places from 1906 on. It was beautiful work. Not all 'beer and skittles' by any means, but beautiful work all the same. It is work, however, that depends absolutely on the perfection of its details ; like aseptic surgery." See also Dr Carter's article [1909] for a brief but clear analysis of the subject.

S. T. Darling, M.D., has kindly informed me that the Panama Anophelines which carry malaria are *A.* (*Cellia*) *albimanus* ; *A.* (*Cellia*) *argyrotarsis* ; *A. pseudopunctipennis* ; and *A. tarsimaculata*. See also his papers [1909, 1910], and Section 65 (4).

By J. A. LE PRINCE, C.E.A.M.

Chief Sanitary Inspector, Panama Canal Zone

43. Anti-malarial Work on the Isthmus of Panama; Technics.—As we approach the tropics, where the rate of growth of vegetation and rainfall increases, the cost of maintaining open ditches and keeping them free from *Anopheles* larvae increases considerably. In the north the greater part of the rainfall occurs at a season of the year when mosquito life is dormant. Well-made surface ditches will need very little attention, and those having a fair grade will often be dry during the greater part of the *Anopheles* breeding season. Here we have conditions that are different, and which are very favourable to the development of mosquito life. Our rainy period lasts for seven or eight months. The ground remains wet for many days at a time, and the dense vegetation prevents evaporation of surface water to a large extent. Most of our ditches contain water continuously throughout the wet season and well into the dry season. It is not unusual to have rain or showers occur during twenty-six days out of thirty. Such conditions keep the vegetation in open ditches growing rapidly, and are favourable to the development of growths of algae. The latter play a prominent part in the development of *Anopheles* larvae. If the ditches are not kept free from vegetation, *Anopheles* larvae will generally be found, and the number thereof seems to increase when algae are present. It is necessary to have as few ditches as we can possibly get along with. The flatter the ditch the more the likelihood of it becoming a propagation area. Also, as we know that the larvae have the habit of collecting in

still water, or in water having a low velocity, it is necessary to keep the ditches of uniform cross-section and free from obstructions. If this be not done larvae from up-stream will probably find safe resting-places at points lower down. At times, when all other insect life has been washed down-stream, these larvae may be seen clinging to the bank of the ditch.

In connection with open ditches, it should be stated that hard showers of short duration are a temporary help in that they clean out the ditches and remove all larvae. Very often, however, hard rains will practically ruin a ditch and make regrading necessary. A most important source of *Anopheles* on the Isthmus is the seepage water[1] that outcrops on hill-sides or on low ground near the foot of a hill. Such productive areas may last for short or long periods, and may occur at irregular intervals. If the quantity of seepage water be small it may not be noted. In case it occurs near a settlement, such an area, even though small, may and often does become the cause of a large increase in the previously low malarial sick-rate; and, moreover, extra attention will probably be given to anti-malarial work at more distant points, while the real source of the trouble may have escaped attention.

Sub-drainage is far superior to open ditches in every way, and should be used whenever conditions are favourable therefor. It is much more economical and better than the application of larvicide under such conditions as exist here, for it is always possible, even with the most careful inspection, for numerous small wet areas to be missed.

The practice here is to cut off seepage water by means of intercepting ditches containing drain tile. In all cases locations and plans of such ditches should be made at the height of the rainy season, when the worst conditions occur. If this is not done errors will surely be made, and during extra long rainy periods seepage water will occur on the up-hill side of the tile line. Ditches for sub-drainage must be

[1] *I.e.*, Water which oozes out.—R. ROSS.

cut to true grade, and should be made as narrow as possible so as to reduce the amount of cover stone needed. The tile should be laid to true grade with open joints of about 1/8 inch, and then be covered with stones corresponding to field stone of about 4 to 6 inches in diameter. The dirt from the trench must be placed on the downhill side of the tile line to prevent it washing back into the ditch. When the soil uphill from the ditch is covered with vegetation the space between the cover stones does not fill up. Water will find its way into the tile line through the joints. We find that the flow of water in drain tile thus laid always carries such fine silt and sand as enters through the joints of adjacent sections, provided the grade is about 1% for 6-inch tile and not less than one half of 1% for 10-inch tile, when there is no loose soil above the line of the ditch. Special care should be taken to have the outlet of the tile kept clear at all times. When tile drains are located and laid properly they are the most economical method of draining for anti-malarial work, and the cost of maintenance is very small. Some drains laid three and a half years ago have not cost one cent for maintenance since installation. Due to washouts, erosion, necessity of frequent regrading, cleaning, removal of vegetable growth, algae, etc., the cost of maintaining open ditches is often expensive and necessarily continuous. If such maintenance costs are or will be excessive, and conditions such as grade, character of soil, etc., are unfavourable for sub-drainage, we make the ditches the shape of a flat "V," and line them with flat stone laid in just sufficient cement mortar to hold them together, leaving sufficient weep holes near the rounded floor to take care of ground water. Frequently here the cost of maintaining an open ditch for two or three years will be more than that of lining the ditches permanently with flat stone. Near some of the camps such lining of ditches costs from 25 to 50 cents per running foot, and as the same ditches cost previously about 25 cents per foot per year to maintain,

the advantage of the permanent work is self-evident. Stone-lined ditches are very seldom sources of *Anopheles*. Filling is sometimes necessary in low lands that cannot be drained, and under certain circumstances it should be accomplished, providing the cost is not prohibitive.

A large number of *Anopheles* larvae may occur on flat lands where water collects in small quantities in numerous places. Although much of the water may evaporate, and the breeding-places dry up before the larvae can develop into adults, yet what usually happens is that a sufficient number of the small wet places remain, which, taken together, will produce during the rainy season sufficient adult mosquitos to keep a near-by settlement well supplied. All very small as well as large wet places that are not thoroughly dry during a period of ten days need attention.

Evaporation of the surface-water can be largely increased by the removal of the vegetation. In fact, in many such areas we find that the larvae develop only when the grass and vegetation are sufficiently high to retard evaporation. Where the water continues to remain after the removal of all vegetation it will often be necessary to apply larvicide. In case the breeding areas cover a large territory, the transportation and proper application of the larvicide may cost several times more than the larvicide itself. Then it becomes necessary to use some form of concentrated larvicide in order that the labourers may employ nearly all of their time in applying it rather than in its transportation. After much unsuccessful experimentation with commercial larvicides it was found that a mixture of carbolic acid, resin and caustic soda has very satisfactory larvicidal properties and acts promptly. It meets the conditions that we have to contend with fairly well. It costs less than one-half of the best commercial larvicide we could obtain, and the latter was apparently not standardised. One part of this mixture placed in five thousand parts of water containing mosquito larvae will kill them all in less than five minutes.

When one part is applied to eight thousand parts of water containing larvae, the same will be killed in thirty minutes.

The method of preparing it is as follows:—Crude carbolic acid, containing about 15% of phenol, is heated to 212 degrees F., finely pulverised resin is added, and the mixture kept boiling until the resin is all dissolved. Caustic soda is then added, and the solution kept at 212 degrees F. for about ten minutes, or until a perfectly dark emulsion without sediment is obtained. The mixture is thoroughly stirred from the time the resin is added until the end. As the composition of crude carbolic acid varies greatly, the proportion of ingredients of the larvicide will vary, and it is necessary to have small experimental lots made in the laboratory, and tested before the batch of larvicide for use in the field is manufactured. The average mixture is about as follows:—300 gallons crude carbolic acid; 200 lbs. resin; 30 lbs. caustic soda. The larvicide costs us about 14 cents a gallon.

In lagoons, ponds and lakes near settlements we get rid of the vegetation so far as possible, and at the edges where the water is shallow, and in other places where larvae occur, we destroy them with this larvicide.

When numerous small particles of dead vegetable matter that are washed into lakes or ponds are collected by the wind, the *Anopheles* lays her eggs in amongst this *débris*, and it must be removed.

In long running streams and open ditches where anti-malarial work is being done crude petroleum may be used, and it is more effective when applied automatically. Should the quantity of running water be small, so that not much oil is needed, then a handful of cotton waste tied in a bundle is soaked in oil for a day, and then placed near where the water outcrops. The water passing by this waste becomes covered with a very thin film of oil. The waste gives off an oil film for about ten or fourteen days, and is then resoaked in oil and used again. For larger bodies of water having a fair

velocity, drip cans or drip barrels are used, and should be located 2 feet above the water surface to break up the oil drops as they fall. They are made as follows:—A piece of metal similar to that of a flat wick lamp that holds the flat wick is fastened to the barrel or can near its base. This wick chamber is made somewhat larger than the wick, so that the wick may fit it loosely when saturated with fuel oil. The space inside the barrel, between the wick chamber and the bottom of the barrel, is filled with a solution of caustic soda or larvicide. As the oil passes along the wick it comes into contact with the caustic soda, and is "cut" or rendered thinner. This prevents the wick from becoming clogged by the thick fuel oil. The wicks seldom clog, but in case such occurs some larvicide is dropped on the wick. This cleans it, and makes it as serviceable as before.

Adult *Anopheles* may persist near settlements when it is supposed that all that is possible has been done toward their eradication. Near the seashore the larvae of some species of *Anopheles* occur in water that is quite brackish. All brackish water should be considered as capable of supporting *Anopheles* larvae until the contrary is proved to be true. Also mud from above which the water has recently disappeared should be looked on with suspicion, and treated when it supports mosquitos. Many small muddy areas may contain no mosquito life, but if the larvae are present in some small spots, the sum total of such possible places, most of which are hidden by the grass and not easily seen, is worthy of consideration. The adults that may develop therefrom in rainy periods are often numerous. Larvae may only be present in one or more particular parts of a large possible breeding-area, and a superficial inspection thereof will be misleading. Dipping with a cup is a very unsatisfactory way to determine the presence of larvae. Even when larvae cannot be dipped up from a small shallow body of water, if the same is stirred up the larvae will often appear promptly at the surface. When no larvae can be seen it does

not always mean that none are present. We use the larvicide application method to determine the presence or absence of larvae to a large extent. When applying strong solutions of larvicide at the edges of a large body of water it is noticed that the larvae appears to lose all sense of direction. Very few reach the clear water, and nearly all that do so die soon afterwards. Our labourers are supplied with knapsack sprayers for distributing larvicide, and a labourer can take enough larvicide along with him to last for several hours of spreading. The cost of spreading a dollar's worth of crude oil by hand often amounts to two dollars or more.

So far as we can determine not much malaria is actually contracted by labourers while outside on the work. In fact we have no case on record where a person has been bitten on the Isthmus by an *Anopheles* when the person was exposed to the direct rays of the sun. Although we think very little infection is conveyed during working hours out in the sunlight, we do know that it is a common occurrence for persons to be bitten in the daytime by *Anopheles* when both are inside of buildings. We have noted that a person indoors may be bitten several times in a period as short as ten minutes, and that *Anopheles* while indoors will take blood at any time between daylight and dark, as well as at night.

In connection with the prevention of malaria, the destruction of adult *Anopheles* in barracks, tents and other crowded quarters is certainly worthy of attention.

The screening of buildings is of importance. To keep the screening intact and effective is quite a problem where ignorant labourers are concerned. A screened building is far superior as a preventative measure to the use of mosquito bars over each cot. The latter, as used by labourers, is generally of little value. The American housewife on the Isthmus is interested in keeping the screened doors closed in order to keep out mosquitos and flies. The children and wives of American employees have been particularly free from malaria, and in fact do not contract malaria

on the Isthmus as often as they would in many parts of the United States.

During February of 1907 a census of American women and children then in the territory between Bohio and Balboa was taken. There were 552 women and 345 children present. Some of them had come to the Isthmus in 1904 and 1905, but the greater part in 1906. The malarial sick-rate in 1905, 1906 and 1907 was very much higher than it is to-day. Among the 897 women and children, 82% had not had malarial fever between the time they arrived here and up to the time the census was taken. During the same period some of the labourers' camps had a 10% malarial hospital sick-rate per week. Of the American children on the Isthmus in February 1907, 90% had not yet had malaria. At Bohio, where the houses were not screened, there were seven American children and seven American women in February 1907. Three of the women and three of the children had had malarial attacks since arrival at Bohio.

A temporary camp consisting of tents was established at Cocoli during April 1908. *Anopheles* breeding-places occurred on all sides, and we could not possibly get rid of them without considerable expenditure of funds. The size of the camp did not warrant much expenditure. Adult *Anopheles* were very numerous about the camp, and thirty or more could be counted in a single tent in the daytime. A watchman was employed to kill off all mosquitos seen in the tents during the daytime. This watchman kept on the go from tent to tent. All mosquitos in the tent were within reach, which was a very important advantage. No work was done by the watchman after 5 P.M. At first *Anopheles* were so numerous that a tent could not be left for a half-hour without some coming in. The near-by breeding-places were oiled in order to reduce the number of adults, but many remained. The destruction of adults began 10th April 1908, and between that time and 24th August 1908 there were seventeen cases of malaria at Cocoli, which would mean only a little over 1% of the force were sick with malaria during each

period of seven days. The camp was abandoned on 24th August 1908.

This was a lower sick-rate than had occurred at other camps where anti-malarial work had been carried out for quite some time, but at Cocoli we were killing adult mosquitos, and the other places were not.

As this work resulted so successfully it has since been carried out at labourers' barracks at various stations, and has undoubtedly been an important item in the marked decrease of the malarial rate that has occurred during the past year.

Recently some six hundred *Anopheles* (mostly *albipes*) were collected alive in various camps where the adult *Anopheles* are being systematically destroyed daily in barracks. They were examined by Dr S. T. Darling, Chief of the Board of Health Laboratory, and not a single one showed malarial parasites. It seems fairly evident that we catch most of the *Anopheles* between the time they first enter the barracks and the time when they should become infective. The catching of adults in the buildings which have high walls or in dark rooms is a much more difficult proposition than their destruction in low tents, where all can easily be seen and reached. It would be a large advantage to have the interior of buildings painted a light colour or white-washed so that *Anopheles* could be readily seen when resting on the walls. We now use test-tubes containing cotton wool and chloroform to destroy adults resting on walls, etc. It is quieter than the slapping method, and very few mosquitos are frightened away.

At the base of Diablo Hill is a swampy area about two miles long and a half a mile wide. It cannot be drained at the present time. From the top of the hill to the water-line is about 200 yards. In June of 1908 several hundred United States Marines were stationed at Camp Diablo, and remained on this hill for about six or eight weeks. The conditions affecting malaria, such as drainage, etc., were then the same as they are to-day. While the Marines were in the camp their malarial sick-rate

was 2% per day, or 14% per week. Since that time a string of railroad cars in which the labourers of the railroad construction gang sleep was located near the foot of the hill, and between the swamp and the camp formerly occupied by the Marines. During the past thirty weeks an average of forty-four *Anopheles* per day have been destroyed each morning in these cars. They were caught in chloroform tubes as above described. It takes the labourer thirty minutes each morning to do this work. The cost is 5 cents per day, or 30 cents per week, and the destruction is carefully performed by a competent man. From the first week in May 1909 to 30th November 1909, which is the rainy season, only four cases of malaria have occurred among the forty labourers sleeping in these cars, or about one man in 300 men working for a period of seven days, although they were more freely exposed to the *Anopheles* than the Marines had been. This work of destruction of adults in quarters costs so little that undoubtedly it will be of value in all malarial districts where house tenants are interested in the prevention of malaria. It will be of special importance where an army is in camp, or where railroad camps are established in malarial districts, as with proper care the sick-rate could by this means be kept far below what it would otherwise be. On the Isthmus we consider such work as supplementary to the drainage and other anti-malarial measures.

It should be noted that most of the labourers' camps in the Canal Zone are located near native towns, and undoubtedly a large percentage of what malaria we do have is contracted by our labourers spending their evenings at, or living at the native settlements. Nevertheless, in spite of continuous topographical changes, constant rains, native villages and numerous other difficulties, the malarial sick-rate is being steadily and systematically decreased year after year. The decrease of adult *Anopheles* to be found at the camps is equally noticeable.

Several years ago it appeared that the malarial sick-rate would always be much higher during the rainy season than in

the dry season, but during the past two years the Department of Sanitation have been able to control the situation to such an extent that at some of the larger settlements there has been no appreciable increase in the number of malarial cases during the wet season as compared with the dry season. It is of interest to note that this has occurred in those districts where during previous years the increase of malaria closely followed the increase of rainfall. This is very encouraging, considering that such results have been obtained where the settlements are scattered over large areas.

With regard to the destruction of adult *Anopheles* in dwellings it should be stated that malaria cannot always be controlled by this means only, although it was fairly successful at Diablo and Cocoli. The colour of the house walls, height of the ceiling, the amount of light in rooms and the number of hiding-places, etc., effect the percentage of adults that may be destroyed. Again, if persons living in houses that are inspected daily visit houses that are not so treated, or are infected elsewhere, the results will not be as satisfactory as where their camp is isolated as was the case at Cocoli and at Diablo. Drainage and eradication of breeding-areas is the *all-important* work in an anti-malarial campaign. The catching of adults should be considered as supplementary to drainage, screening, application of larvicide, etc. At Havana we relied exclusively on drainage and filling. Most of the natives there do not like to take quinine. Malaria at Havana has practically been eradicated.

Data relating to charts showing reduction of malaria in the Canal Zone.—Chart No. 1 shows the percentage of all Canal employees (malarial cases only) that were sent to the hospital each month from 1906 to 1909 inclusive. Attention is invited to the fact that the sick-rate is always lowest about May or at the end of the dry season. That the malarial sick-rate would advance rapidly in former years with the arrival of the wet season June, July and August. This advance has been materially checked. Compare the wet season record of 1909 (June to December) with the same period of the previous years

and with the dry season of 1909 (January to May). The successive yearly reductions are a fair index of the value of the anti-malarial work.

The successive months are plotted one half inch apart, horizontal measurement.[1] The percentage malarial sick-rate is plotted on the vertical lines, on a scale of two inches vertical measurement, equals 1%. If the force employed consisted of 40,000 men, and 500 of them are sent to the hospital with malaria during a month, then the rate on Chart No. 1 for that month would be plotted as 1·25%.

Chart No. 2 is a record of malarial cases among employees at Porto Bello. The percentage sick-rate for each week plotted. This was a new camp that was opened up a few weeks before anti-malarial work started. At all newly opened camps the malarial rate runs high, as compared with areas where anti-malarial work is being done. Without doubt the rate would remain high if no anti-malarial work were done. Note that preventive work at Porto Bello started 21st March 1908, and that the rate from then to June averaged, say, about 3% per week. Or, over 12% of employees were sent to hospital per month (malarial cases), and many of the labourers remaining at work were not in physical condition to do as much work as they are capable of doing to-day. That during this same period, among employees in the Canal Zone, the monthly sick-rate (malaria only) was not much over 1%. (See Chart No. 1.) About 600 men are located at Porto Bello.

Chart No. 3 is a record of malaria at Gorgona. Present population 2,000 employees. Compare years 1908 and 1909 with the previous years. No large increase of malaria occurs in the wet season (June to December) during the last two years. *Anopheles* are now seldom found in barracks, which indicates that they are not numerous. Note the effect of the wet season on the malaria rate for 1906, and absence of same for wet seasons of 1908 and 1909.

In Charts No. 2 and No. 3 the weekly sick-rates are given.

[1] The accompanying charts are reduced.—R. ROSS.

For instance, during the week ending 6th January 1906 a number of the employees at Gorgona (Chart No. 3) were sent to the hospital whose cases were diagnosed as malaria. This number consisted of 2·74% of the entire working force living at Gorgona during that week. The dates of the ending of each week for which the percentage sick-rate is plotted are given. These percentages show only the malarial sick-rates and not the other hospital cases.

In connection with Chart No. 1 it should be stated that in the early days it was difficult to get the malarial patients from among the ignorant labourers to go to the hospital, and so the actual sick-rate at that time was higher than what has been recorded. At the present time the labourers go voluntarily to the hospital when they feel sick. Note that during seven months of 1906 the rate was above 6%; that during 1907, for ten months, the rate was below 5%. In 1908, during eleven months, the rate was below 4%. In 1909, the rate was below 3%. The highest monthly rate during the wet season of 1909 was a little over 2%, as compared with 4%, 5% and 10% respectively for the wet season of the years 1908, 1907 and 1906.

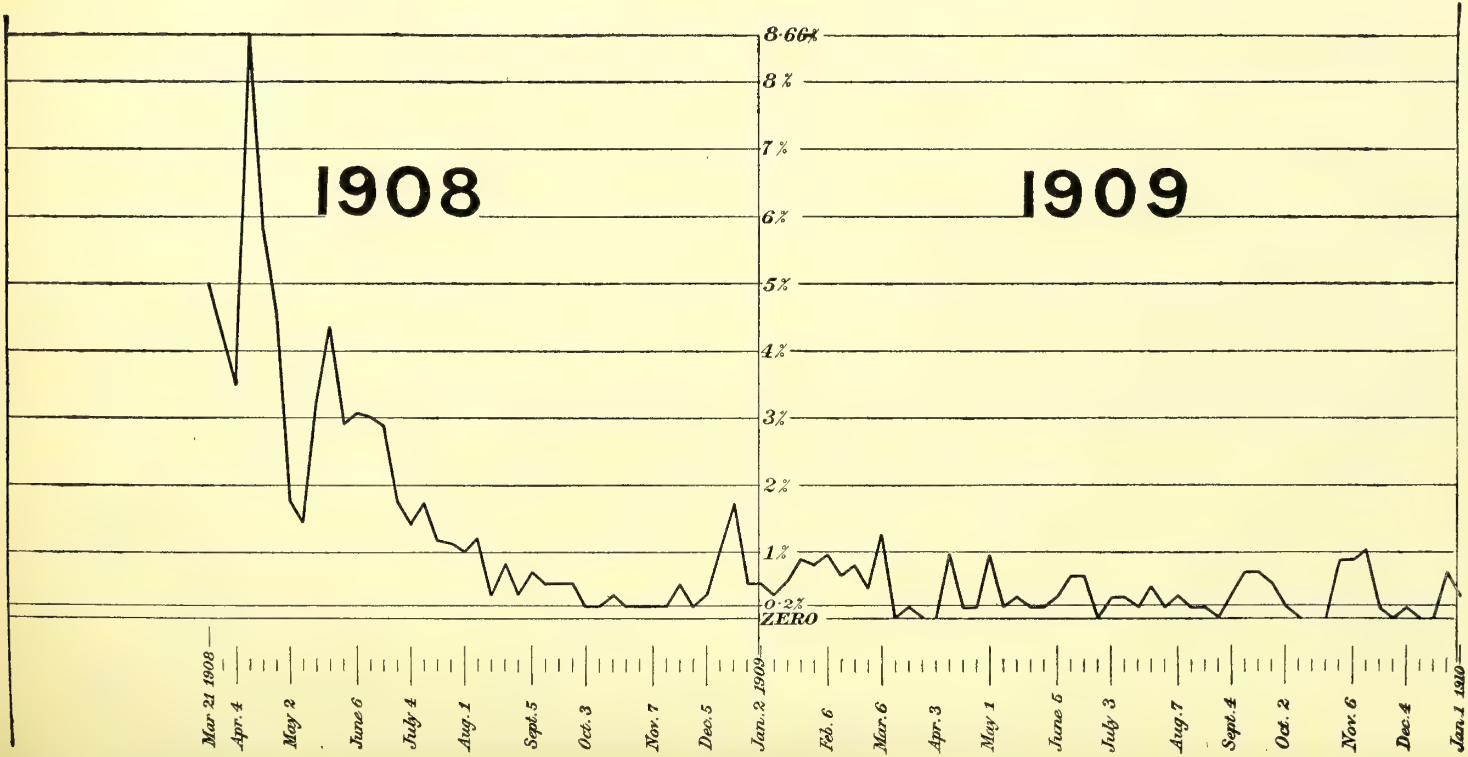
Refer again to Chart No. 2, relating to malaria at Porto Bello. During December of 1909 the number of rainy days was twenty-seven out of thirty-one. The rainfall was 45·03 inches for November and 58·17 inches for December 1909, as against 41·73 and 25·72 inches respectively for November and December of 1908. Such conditions of rainfall mean a large increase in the number of possible *Anopheles* areas. As shown on the chart, no increase of malaria occurred due to the increased wet area accessible to *Anopheles*.

This is a good illustration of the results of anti-malarial work performed under severe conditions when the work might at first sight appear to be almost impossible to accomplish. From 1st November 1908 to 2nd January 1909, 5% of the force were sent to hospital with malaria. From 31st October 1909 to 1st January 1910, during the period of heavier rainfall, only 3·26% of the force were hospital malaria cases.

This Chart shows the number of malaria cases sent to hospital each month, expressed as a percentage of the entire working force

CHART No. 2.—MALARIA AT PORTO BELLO, R.P.

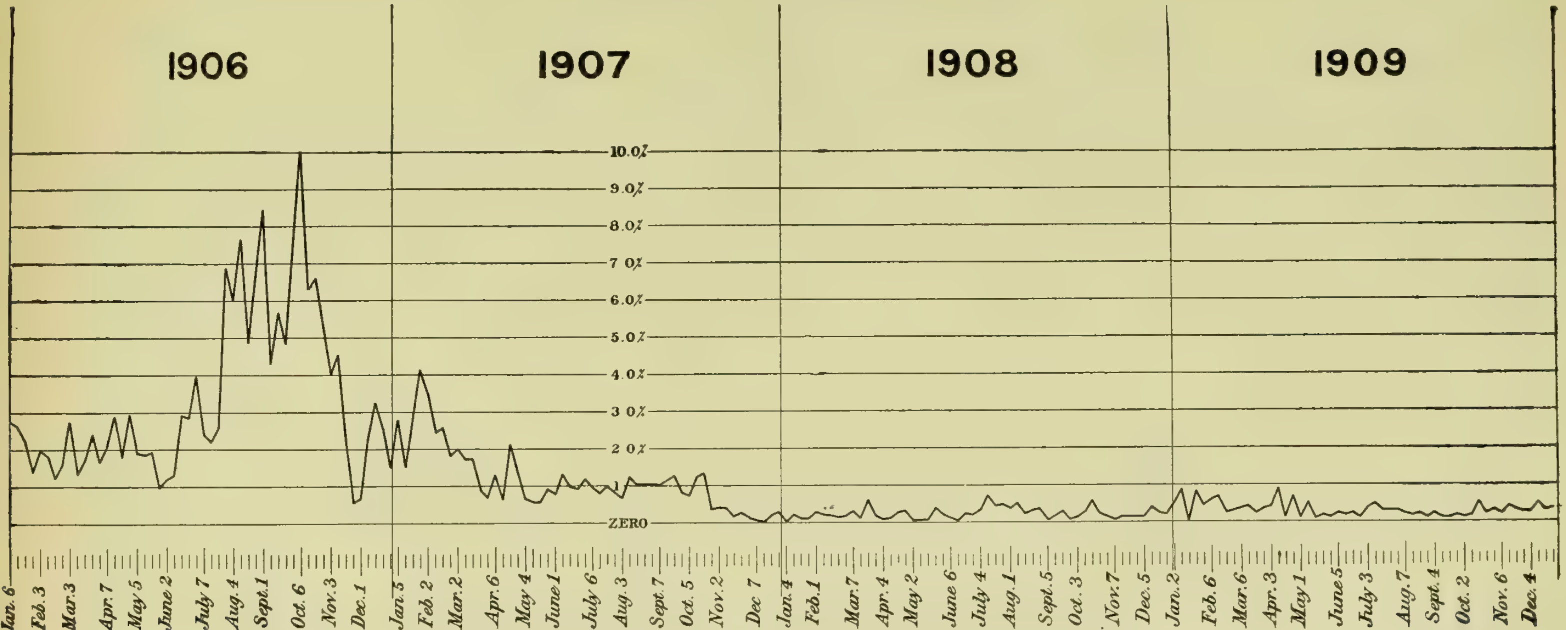
HOSPITAL CASES.



The Chart shows the percentage of employees of the Isthmian Canal Commission sent to hospital each week, and shows the reduction of the

CHART No. 3.—MALARIA AT GORGONA, C.Z.

MALARIA CASES SENT TO HOSPITAL FROM GORGONA.



The Chart shows the percentage of employees of the Isthmian Canal Commission sent to hospital each week.

BY SIR RUBERT BOYCE, F.R.S.

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44. Malaria in the West Indies.—There is no doubt that malaria has markedly decreased throughout the West Indies. This statement can be verified by comparing the current health reports with those furnished by the Surgeon-Generals when the various islands were garrisoned. The factors which have brought about this diminution are the same as those which have, in more recent time, operated in towns like Bombay, New Orleans, Rio, etc., and which at a still earlier period led to the disappearance of malaria from a large portion of Europe.

It may now be stated that malaria as an endemic disease is limited to the country districts throughout the islands, and that it ceases at the outskirts of the principal towns, in a few instances penetrating into a centre of population along the banks of some neglected marsh or stream.

In the principal towns the swampy ground which almost invariably surrounded the early settlements has now all been reclaimed, and in its place well-planned streets have appeared. Properly-graded roads and concrete drains are the rule rather than the exception. Since the year 1850 a considerable proportion of the larger towns received the immense advantages of a pipe-borne water-supply, derived from reservoirs or rivers outside the towns. This system at once did away with the old-time wells, and abolished, or greatly decreased, rain-water barrels and storage tanks. These improvements, coincident

with the progress of prosperity and the appreciation of the laws of hygiene, have together brought about the cessation of malaria and yellow fever as endemic diseases in the chief towns. Therefore it is to the country districts, and on the plantations, that the student must look for the survival of malaria in its endemic form. And there is no doubt that it does still so survive throughout the Antilles, as an analysis of the Registrar-General's or the Medical Officer's Mortality Returns abundantly proves. Unfortunately, it is very difficult to estimate accurately the total sickness and mortality rates from malaria in the various islands ; in many instances the causes of death in 50% or more of the cases is not definitely stated. And, of course, as is well known, although the anaemia of malaria may have had the chief share in producing lowered vitality, yet the actual cause of death is registered as perhaps bronchitis or consumption, as the symptoms of these affections were the more obvious ones from which the patients suffered during illness.

Efforts are now being made, however, to make more accurate estimates of malaria by the employment of more perfect methods of diagnosis, such as spleen rates and blood examinations.

Grenada.—Population, 68,253. Capital, St George's ; population, 5,198.

Malaria is still prevalent in the country districts, and is endemic and imported. In No. 1 District in St George's Parish 681 cases and 22 deaths were recorded in 1906, and 317 cases and 12 deaths in 1907. In No. 6 District 1,119 cases were reported in the same year. The malaria is due to the breeding of Anophelines in certain swampy lands and puddles, and, in the wet season, perhaps also to the presence of water-holding epiphytes upon the shade trees on the cocoa plantations.

Preventive Measures :—

1. Introduction of pipe-borne water. This was the first step which led to the reduction of Anophelines in the

towns of St George's, Gouyave and Grenville by doing away with numerous barrels and odd water containers.

2. Road-making and street drainage in the towns.
3. Government and medical officers throughout the colony making returns of all anti-malarial measures undertaken in their respective districts.
4. 1st April 1909.—The fish "millions" were introduced into the colony with the object of stocking ponds and large collections of water.
5. The Public Health Ordinance of 1907 prohibited the keeping of stagnant water unless protected, oiled or stocked with fish, and all odd receptacles were to be removed.
6. In 1902 and 1905 further ordinances were promulgated, declaring mosquito breeding-places in and about human habitations to be a statutory nuisance.
7. The more careful cultivation on the cocoa estates has led to better drainage, and a diminution of malaria on the estates.
8. Considerable attention is now paid to entomology.

St Vincent.—Population, 47,548. Capital, Kingstown; population, 4,547.

Malaria for the most part confined to small, swampy districts in the country. The Medical Officer's returns show 525 cases in 1907, and 281 cases in 1908.

Prophylaxis :—

1. Pipe-borne water has led to great diminution of water barrels and odd receptacles and wells.
2. Construction of side drains and roads.
3. Removal of odd receptacles from yards, and better sanitary supervision.
4. *Anti-larval measures*—

1901.—Measures promulgated against stagnant water, unless oiled, screened or stocked with fish.

1909.—New Public Health Ordinance, with provision against stagnant water and mosquito larvae.

5. The late Dr Branch was a keen entomologist, and did much to get rid of breeding-grounds of mosquitos.

St Lucia.—Population, 50,000. Capital, Castries.

There is still a small amount of malaria, due to a few low-lying, swampy patches which persist near Castries.

Prophylaxis :—

1. Introduction of pipe-borne water brought about a great reduction in the number of breeding-places.
2. Better roads and side drainage undertaken from 1890-1900.
3. Energetic anti-mosquito drainage and bush clearing operations undertaken by Major Hodder.
4. *Anti-mosquito laws.* — In 1906 the Colonial Secretary commenced a vigorous systematic campaign against mosquitos, by causing careful returns to be made of all insect-carried diseases and of the breeding-places of mosquitos.

In 1907 regulations were framed dealing with stagnant and waste water.

In 1909 a vigorous house-to-house campaign amongst mosquitos was started.

Fines have been regularly enforced from 1907 against all who, after caution, refuse to get rid of mosquito larvae.

Trinidad.—Population, 336,769. Chief Town, Port of Spain ; population, 70,000.

General death-rate for 1908, 30 *per mille.*

Total deaths from malaria, 71 in 1908. Total amount of malaria small.

Upon the estates, however, malaria is responsible for more than half of the gross sickness rate amongst the indentured labourers.

Prophylaxis :—

1. *Pipe-borne water-supply.*—This system was first introduced into the Port of Spain as far back as 1851, and has since then been largely extended throughout the Port of Spain. To it must be ascribed the abolition of endemic yellow and malarial fevers, for it at once did away with the necessity of the numerous water barrels, cisterns, wells and odd receptacles of all descriptions, which in the old days were the breeding-places of the mosquitos.
2. The construction of well-graded roads and concrete drains everywhere within the vicinity of the town.
3. Concreting backyards, removal of rubbish and bush.
4. *Education.* — Systematic training of the sanitary inspectors, schoolmasters and others throughout the colony.
5. *Entomology.* — The establishment of a strong entomological department to study the breeding-places of mosquitos.
6. *Anti-larval laws.* — In 1907 regulations were made against stagnant water and larvae, enforcing the screening or oiling or fish stocking of stagnant water which could not be got rid of.

   In 1909 a new Draft Ordinance was prepared strengthening these regulations, and numerous fines have been inflicted for contravention of the Mosquito Bye-laws.

British Guiana.—Population, 278,328. Capital, George Town.

Colony intersected by innumerable water trenches, swamps and canals.

Malaria, the principal disease of the colony.

In 1907 the deaths from malaria were 301.

Prophylaxis :—

1. Construction of good roads and drains.
2. Pipe-borne water-supply to George Town not completed.
3. Fish stocking of all waterways.

4. Education, and training of sanitary inspectors, teachers, schools, etc.
5. Sanitary supervision, removal of odd water receptacles.
6. *Drainage Ordinance.* — In 1907 Bye-laws were introduced enforcing proper drainage in George Town; later these were extended to the country districts.

   Vat Screening.—In 1907 an Ordinance was introduced to render compulsory the screening of vats, and this law was enforced in 1909.
7. The distribution of quinine.

Barbados.—Population, 199,542. Chief town, Bridgetown.

It is stated on good authority that there are no Anophelines present in this island.

Malaria is not endemic; imported cases of malaria are common amongst the labourers returning from Panama. The absence of Anophelines is attributed to the very porous nature of the soil throughout the island, which prevents the formation of puddles. The ponds which are met with are all apparently stocked with "millions."

In Bridgetown the streets are well drained and kept flushed.

Water-supply. — An abundant and excellent pipe-borne water-supply was introduced into the colony many years ago, and has since been extended. It strikes at the root of endemic yellow fever by putting an end to the innumerable odd water receptacles which were everywhere abundant.

In 1909 a vigorous anti-mosquito compaign was started, and the Anti-stagnant Water Ordinance of 1909 was rigorously enforced.

Sanitary inspection was increased, and a general cleansing and removal of all odd water receptacles carried out.

The Bahamas.—In 1905 measures were adopted against mosquito-carried diseases, and in 1907 anti-larval regulations were enacted, and screening, oiling or fish stocking of stagnant water enforced. Quinine was also distributed.

The Northern Islands.—In 1904 vigorous action was commenced against mosquitos.

Jamaica.—Population, 639,491. Capital, Kingstown.

Anti-malarial measures in Jamaica.—In October 1909 a special commission was appointed to investigate and to take measures to remedy the conditions which gave rise to malarial fever in different parts of the island of Jamaica. The Commission consisted of the Colonial Secretary, P. C. Cork, Archbishop Nuttall, the Superintending Medical Officer, Dr Kerr, Dr Turton and Dr Graham, and Messrs Gideon and Hart. Mr Isaacs was made Official Secretary of the Commission.

The objects of the Commission were to ascertain the breeding-places of Anophelines, and to suggest the carrying out of remedial and preventive measures; also to conduct investigations and to take evidence if necessary. The Commission had power to expend up to £500.

British Honduras. — Chief town, Belize; country very swampy.

Prophylaxis :—

1. Anti-mosquito work was commenced in 1905, and has been kept up with marked success.
2. A mosquito destruction Ordinance was passed in 1906. Owners of property were compelled to screen, oil or stock with fish all collections of water.
3. Drainage and filling - in operations have also been started.

Bush clearing was enforced.

By W. T. PROUT, M.B., C.M.G.

Lately P.M.O. Sierra Leone

45. Malaria in Jamaica.—The chief factors which influence the prevalence and distribution of malaria in Jamaica are, first, the physical configuration of the island, and second, the methods of cultivation, which are necessitated by the requirements of its forms of agriculture.

The centre of the island is occupied by a lofty chain of mountains trending generally east and west, from which spurs are thrown off running north and south, with intermediate fertile valleys: while the coast is occupied by flat alluvial land of varying extent. From the mountains a number of streams, some of considerable size, rush precipitously down till they reach the lower plains, where they frequently form extensive grass-grown swamps and open into the sea by a series of shallow, sluggish mouths, in all of which malaria-carrying mosquitos breed in great abundance.

Moreover, it is naturally in the alluvial plains that the staple industries of the island, namely, banana and sugar plantations, are carried on. In the north-eastern portion, where the land is almost entirely under banana cultivation, there is a heavy rainfall, and the soil is stiff and clayey. In consequence, the banana

plantations have to be deeply trenched for the purpose of subsoil drainage, and the trenches, unless properly graded and kept clear of grass and weeds, afford suitable breeding-grounds for Anophelines.

In the south and south-eastern parts of the island different conditions obtain. The rainfall is small, and irrigation, both on banana and sugar estates, is imperative. Here again the canals, unless properly supervised, are a prolific source of mosquitos.

Among minor factors may be mentioned the ponds formed by surface drainage, invariably grass-grown, and used sometimes as the water-supply of a village, and more frequently as cattle ponds; and the shallow, earthen, weed-covered gutters along the sides of the streets of the villages and towns.

General malarial death- and sick-rates.—Although the extensive prevalence of malaria in certain districts of Jamaica has long been recognised, and its seriousness as affecting not only the general health and the death-rate of the community, but the agricultural development of the colony, was fully appreciated by the medical profession, yet, apparently from the apathy of the laity, no attempt appears to have been made to make an accurate malarial survey of the island, and preventive measures were practically non-existent. In the latter part of 1908, however, an expedition from the School of Tropical Medicine of Liverpool visited the island, and, thanks to the active co-operation of the Government and of the Island Medical Service, a systematic though limited survey was made, and much useful information was obtained. The following statistics are mainly taken from one of the reports of the expedition by myself (*Ann. Trop. Med.* III., No. 4, 17th November 1909).

The total malarial deaths for the whole island for a period of ten years amounted to 34,695, which is equivalent to a malarial death-rate of 4·4 per 1,000. The general death-rate during the same period was 22·5. The average percentage of malarial to total deaths was 19·7, representing nearly one-fifth of the total deaths.

A summary of the statistics is given in the following table:—

AVERAGE DEATH-RATES, ETC., FOR THE DECENNIUM ENDING 30TH APRIL 1907.

Parish.	Average death-rate from malaria.	Average death-rate from other causes.	Average death-rate from all causes.	Average percentage of malarial deaths to total deaths.
St Thomas .	6·5	18·5	25·0	26·1
St Catherine	6·2	19·4	25·6	24·4
Westmorland	5·9	15·8	21·7	27·7
St Mary .	5·9	18·0	23·9	24·6
Clarendon .	5·3	15·2	20·5	23·8
Portland .	5·3	19·3	24·6	21·9
St James .	4·8	17·1	21·9	22·3
Hanover .	4·8	19·5	24·3	19·9
St Andrew .	4·0	23·9	27·9	14·6
St Ann . .	3·4	14·5	17·9	19·4
Trelawny .	3·4	20·8	24·2	14·4
St Elizabeth	2·9	15·6	18·5	15·8
Kingston .	2·4	26·3	28·7	8·6
Manchester .	1·6	14·8	16·4	10·0
Whole island .	4·4	18·1	22·5	19·7

But the prevalence of malaria is really much greater than is indicated by the death-rate, for of the admissions to the Government hospitals of the colony, it is found that at least one-third are due to malaria, and the malarial sick-rate shows a marked increase of recent years.

Year.	Total admissions from all causes.	Total deaths.	Death-rate per cent.	Malarial admissions.	Malarial deaths.	Malarial death-rate per cent.	Percentage of malarial to total admissions.
1904-05	16,103	669	3·7	4,827	89	1·8	29·9
1905-06	17,856	563	3·1	6,285	88	1·4	35·1
1906-07	21,555	661	3·06	7,113	99	1·3	32·9
1907-08	21,837	830	3·8	7,510	121	1·6	34·3
Total .	77,351	2,723	...	25,735	397	...	...
Average	19,337	680	3·5	6,433	99	1·5	33·2

It is calculated that the cost of the maintenance and treatment of the malarial cases alone amounts to over £6,300 per annum.

On banana and sugar estates the loss of time through illness, mainly malarial and therefore preventable, amounted to sixteen out of every hundred working days, per coolie labourer employed; while among the constabulary, a picked body of men, it is estimated that there is a loss of nearly four thousand days' service per annum from the same cause.

The spleen rate.—That the endemic index of the island is high is shown by the results of the examination of the spleens of children. Among 2,036 children examined in all parts of the island, 536 were found to have enlarged spleens, a spleen rate of 26·3%. But if the mountainous centre of the island were to be excluded, and places on the littoral only taken, the spleen rate would be found to be much higher, and in certain localities rises as high as 80%. The following table summarises the results of the splenic survey:—

Parish.	No. of children examined.	Spleens.[1]				Total No. of enlarged spleens.	Spleen rate.	Average spleen.
		1	3	6	9			
Portland . .	291	112	152	26	1	179	61·5	2·5
St Mary . .	398	216	158	22	2	182	45·7	2·1
St Thomas .	44	27	10	6	1	17	38·5	2·3
St Catherine .	212	156	48	7	1	56	26·4	1·6
St Elizabeth .	249	195	47	7	0	54	21.2	1·5
Kingston . .	220	197	23	0	0	23	10·4	1·2
Westmorland .	278	255	17	5	1	23	8·2	1·2
Trelawny . .	189	187	2	0	0	2	1·06	1·03
St Ann . . .	69	69	0	0	0	0	0	1·0
Manchester .	42	42	0	0	0	0	0	1·0
Chapelton . .	44	44	0	0	0	0	0	1·0
TOTAL .	2,036	1,500	457	73	6	536	26·3	**1·2**

A complete splenic survey, in order to obtain an accurate idea of the distribution of malaria, is one of the first steps to be taken in any anti-malarial scheme.

[1] See section 31 (4).

Anti-malarial measures.—To diminish this extensive prevalence of malaria the following measures have been recommended as being particularly applicable to the requirements of Jamaica:—

1. To make the harbouring of mosquito larvae in compounds a punishable offence.
2. To keep all margins of rivers and swamps in the neighbourhood of towns free from grass and weeds.
3. The application of crude kerosene where possible.
4. The screening of wells, tanks, etc.
5. The cementing of gutters in towns.
6. The gradual reclamation of swamps.
7. Screening of public hospitals, police stations and coolie barracks.
8. Prophylactic administration of quinine to police, coolies, school children and the general public.
9. Education in sanitation and hygiene, all of which would be under the supervision of a central anti-malarial organisation.

As already stated, up to the beginning of 1909 little in this direction has been done. Small quantities of quinine had been distributed to estates for prophylactic purposes, but had not been given systematically and continuously; a few isolated efforts at swamp reclamation had been made by private individuals, and one police station had been made mosquito-proof, but there was no concerted or systematic effort to stamp out malaria.

As a result, however, of the attention which was drawn to the subject by the expedition from the Liverpool School, probably assisted by a severe outbreak of malaria which occurred in the spring of 1909, public interest appears to have been awakened, and in the autumn of that year the first steps were taken to deal with the disease in an organised and systematic manner. His Excellency the Governor appointed a Commission, consisting of the Colonial Secretary, the Archbishop

of the West Indies, the Chief Medical Officer, and other medical men, to enquire into the subject, and to take such steps as appeared to be urgently necessary. A series of public lectures have been given, which has done much to spread a knowledge of the disease in different parts of the island; a new Public Health Ordinance has been passed; steps have been taken to train sanitary inspectors in the recognition of the breeding-places of Anophelines, and in anti-mosquito measures; and, most important of all, in February 1910 a sum of £5,000 was voted to carry out the various measures necessary. So that now we may consider that the anti-malarial campaign in Jamaica has been fully started, and the outlook for the gradual extinction of malaria in the island is distinctly hopeful.

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46. Malaria in the Amazon Region, and the Protection of Ships.—(1). Malaria prevails throughout the *Amazon region.* The chief industry of this part of South America is rubber, and the immense trade requires a large number of men to collect the crop. The conditions under which they work cause many privations and expose them to infection. The old saying that every kilo. of rubber represents the loss of a life is an exaggeration but there is an enormous mortality amongst the "caucheros" who work in the interior.

In the vast territory watered by the Amazon River three places of importance occur, which serve as the commercial centres for the up-river traffic of the interior. Iquitos, 2,200 miles from the mouth of the Amazon, is the only large town in Peru on the eastern side of the Andes. Manáos, on the Rio Negro, some 900 miles distant from the mouth of the Amazon River, and Pará, which lies near the mouth, are in Brazil, and possess the only hospitals for the accommodation of the up-river patients. No attempts have been made by the states of Amazonas and Pará to create any hospitals along the rivers in the interior of the rubber districts. They are very necessary, but their establishment is hindered by reason of the enormous expenses involved.

Iquitos has no pipe-borne water-supply, and the drainage is of the most primitive character. Extensive swamps extend in and about the town, but no attempt has been made to fill them.

Cellia albimana[1] and *Cellia argyrotarsis* abound. The majority of the cases appear to have contracted malaria up-river.

The cities of Manáos and Pará receive many cases of malaria from the interior, and consequently their death-rates are unduly high. Both cities suffer from the return of the infected rubber-collectors, who go and live in the suburbs along the swamps; they either neglect all treatment, or take an insufficient amount of quinine, and, living in a state of poverty and misery, they lie in their mud huts exposed to the bites of numerous mosquitos. Their blood contains many gametes, and they therefore serve as infecting agents for the Anophelines that feed on them. Their presence is a menace to the community, as they may cause most virulent epidemics of malignant types of the disease.

Manáos is intersected by many creeks and swamps. Those in the centre of the city are completely filled; but, on the outskirts of the city proper, certain creeks and swampy areas still form foci of malaria. In the suburbs extensive swamps exist, and very severe forms of malaria prevail. A modern system of drainage and water-supply exists, and has done much to mitigate the evils.

COMPARISON OF MALARIA INDEX OF CHILDREN IN SUBURBS AND CITY[2]

	Spleen-rate.		Blood-rate.	
	0-5 years.	5-10 years.	0-5 years.	5-10 years.
Swamps in Suburbs — Apparently healthy	36·23	42·74	48·93	50·00
Swamps in Suburbs — Ailing . . .	46·82	45·83	51·80	57·14
Swamps in Suburbs — Average both classes	43·07	44·36	50·61	52·60
City swamps	12·94	35·86	29·23	48·78
Children living along swamps in suburbs	43·82		51·64	
Children living along swamps on outskirts of city . .	24·85		40·13	

[1] Newstead, R., and Thomas, H. Wolferstan: "The Mosquitos of the Amazon Region," *Annals Trop. Med. and Parasitology*, vol. iv. No. 1.

[2] Thomas, H. Wolferstan: "The Sanitary Conditions and Diseases prevailing in Manáos, North Brazil, 1905-1909," *Annals Trop. Med. and Parasitology*, vol. iv. No. 1.

The state and municipal authorities are gradually destroying the Anopheline breeding-places within the city. Free advice and medicines are given to the poor, but no steps have been taken to organise a campaign against the hordes of *Cel. albimana* and *argyrotarsis*, which, especially the former, are the carriers of malaria throughout the whole of the Amazon region.

At Porto Velho on the Madeira River, a railway is being constructed to connect Bolivia and Brazil. Several attempts have been made to build this line, and each time malaria has raged amongst the workmen. A most virulent form of fever occurs which has at times incapacitated 50% to 80% of the total working force. The difficulties in combatting the disease are great, and are increased by the possibilities of importing yellow fever from Manáos, Serpa or Pará. The line is in the interior, through virgin forests and large swamps; an organised attack on the lines of the anti-mosquito campaign of Panama and Xerem in South Brazil would facilitate the progress of the undertaking. A graphic account of some of the past unsuccessful attempts is recorded by Craig.[1]

Belem or Pará possesses a pipe-borne water-supply. The swamps are numerous above the outskirts of the city. In 1909 the authorities were influenced to undertake the filling of much swampy land, and to institute free distribution of quinine. A most serious outbreak of malaria occurred amongst the poorer inhabitants living in the outskirts of the city, along the swamps of the Marco de Legua, Pedreira and Canudos, etc. A medical commission [2] traced the outbreak to the arrival in Pará of many labourers who had returned in an infected condition from the Madeira-Mamoré railway and Alcoboça works. These people went to the outskirts, and, living in a state of squalor, they remained untreated, and quickly infected the numerous Anophelines of the locality. Practically every one in the district

[1] Craig, Neville B.: "Recollections of an Ill-fated Expedition to the Head-waters of the Madeira River in Brazil." (Lippencott.)

[2] Relatorio apresentado pala commissão nomeada para debellar a epidemia de pialmudismo, reinante no Marco de Legua, Pedreira e Canudos. Belem. Imprensa official do Estado do Pará, 1909.

uffered from malaria. Free clinics were opened, and in four nonths nearly 13,000 people were treated with quinine; the ery severe cases were sent to the hospital. Endeavours were nade to have all patients with enlarged spleens confined in he hospitals, or screened from mosquitos. Mosquito-nets were ent and all gametocyte carriers were energetically treated with uinine. A brigade was formed to fumigate the dwellings of adly-infected individuals, and a general purgation of the surounding yards and houses was made. Measures were taken o drain and fill the pools and swamps of the most heavily nfected districts, and as many breeding-places as possible were estroyed. A great and general improvement was noted after few months. The number of infections and the number of Anophelines diminished, and it is to be hoped that these satisactory results will encourage the authorities to continue their fforts. The expenses were about £9,500, which was expended n clinics, hospital accommodation, drugs, draining and filling f pools, swamps, etc.

The inhabitants of the Amazon region take very little care o avoid contracting malaria. They frequently neglect to take uinine, or discontinue it after a few days of treatment. The abouring-class Brazilian hardly ever uses a mosquito-net, and, nfortunately, Europeans are prone to follow their example. In quitos, the Peruvian labourer generally protects his bed, not ecause of malaria, but for relief from the swarms of *Mansonia itillans* and *Culex fatigans*, which are veritable pests; this helps o account for the less pronounced infection of the Peruvian.

All through the Amazon region, where large engineering vorks have been undertaken, the old familiar story has been epeated. Not only has money been wasted, but many lives ave been sacrificed by the neglect of the promoters to realise he dangers of malaria and Anophelines. The astute commercial nan has still to learn that it is cheaper as well as quicker to rganise a scientific force to combat malaria or yellow fever, and o have such a force in operation before any attempt is made

to import labourers and commence the work. The Federal authorities insert clauses regarding quinine prophylaxis, screening, etc., in the contracts with the concessionaires, but these clauses are frequently neglected. Many important engineering works are certain to be undertaken in the opening up of the country in North Brazil, and an endeavour should be made to prevent the serious loss of life which has hitherto occurred. The sanitary authorities of both the Amazon and Pará states are alive to the necessity of combatting the disease by extermination of the Anophelines.

(2). *Protection of ships.*—Very little attention has been devoted to the protection of the crew and passengers of steamers navigating regions where malaria prevails. On some steamers mosquito-nets have been provided for the bunks, and a few wire-screened doors for the outside cabins, wheel-house and chart-room. These are inadequate, as they do not protect the entire living quarters of the passengers and crew.

The screening of a steamer is a difficult undertaking. Four facts must always be remembered.

(1) That the navigation and working of the ship should not be interfered with.

(2) That the scheme of screening shall be as simple as possible. Elaborate designs are certain to fail, as they cause much trouble and inconvenience to those on board. The members of a crew or even passengers will neglect to carry out complicated methods of closing doors and port-holes.

(3) That the maximum amount of air shall be available. In the tropics the closeness of the atmosphere does not encourage the average individual to reduce the ventilation by the interposition of wire-gauze screens. I have seen on a steamer plying to the Pacific large screened wooden frames, designed to fit in the port-holes, but so massively constructed that the cross diameter was reduced nearly one-half. It is hardly

to be expected, since so much air and light is cut off, that the crew will make use of the screens.

(4) That an endeavour should be made to screen the vessel in sections, so that if mosquitos succeed in obtaining an entry, they can only circulate in that part of the ship and can be hunted down and destroyed.

Mosquitos generally board a ship when it is moored near the shore or is hugging the banks, and they quickly find their way to the saloons and living quarters of the crew.

All parts of a ship which communicate with the danger zone, *i.e.*, the open deck, should be protected. The doors should be screened, and where there is much passing to and fro between a protected area and the danger zone, a second door should be placed some 3 or 4 feet away, thus forming a vestibule, and allowing one door to be closed before the other is opened. All doors should be provided with spring-locks which catch easily, and a strong door-spring to ensure a rapid and tight closing of the door. If the storm-doors are left on, care should be taken that the knobs do not impinge upon the wire screens and so tear or fray the gauze.

The ventilators leading to the cabin, alleyways, etc., should be screened at the ceiling vent.

Through the courtesy of the Booth Steamship Company the plans of a vessel specially screened against mosquitos are shown. The ship is designed for use in ocean and river-trade, in a district severely infected with malaria and Anophelines. The port of call is far up-river, and is notorious for the severity of the malarial infection. It is the outcome of the experience gained by Dr Melville Davidson, Medical Superintendent of the Company, to whom I acknowledge my indebtedness.

All the port-holes of the steamer are provided with removable screened frames, which are so adapted that the port-hole can be closed and screwed down without necessitating the withdrawal of the screens. The frame of the screen consists of a metal hoop, on which gauze is tightly stretched and soldered. The

frame is provided with rigid bayonet catches, which are so arranged that they slip easily into the ordinary port, and a half turn of the screen is sufficient to lock it.

All the ventilators, as is seen in the drawings (figs. 1-4), are screened by a most simple and efficient arrangement. Ordinary circular moulding is fastened on the ceiling around the vent pipe; the moulding has a deep groove, into which fits a round metal frame covered with wire-gauze; the screened frame is retained in position by three small buttons, so that it can be instantly applied or removed.

The screen doors have light wooden frames and panels of wire-gauze. They are provided with spring-locks and strong coiled wire door-springs.

By reference to the accompanying plan (figs. 5, 6) it will be seen that the arrangement of the screening is quite simple and yet adequate. The screened port-holes and doors are outlined in black. The only entrances to the saloon, chart-room, captain's cabin, pantry and bath-room, are through a screened door on the port side, another on the starboard side, and a third at the foot of the stairs leading from the wheel-house. The outside cabins are all shown with screened doors and port-holes. Further aft, and isolated by the steam-steering gear, are the cabins of the doctor and hospital.

On the main deck the entrances on the port and starboard sides through which the crew must pass to their work are screened. On the starboard side there are two sets of screened doors along the alleyway. These are provided because the ash-discharger is open to mosquitos and therefore constitutes a danger zone. On the port side this extra screening is unnecessary, as in the river only one ash-shoot will be used.

A study of the plan will show that the only unscreened parts of the ship communicating with the interior are the engine-room, stoke-hole and galley. Theoretically, these sections should be screened, but two factors must be taken into account, viz., the intense heat of the places, which is always accentuated in the

S.S. "VINCENT."

PART SECTION OF SHIP SHEWING MOSQUITO PROTECTION TO DOORS AND PORTS.

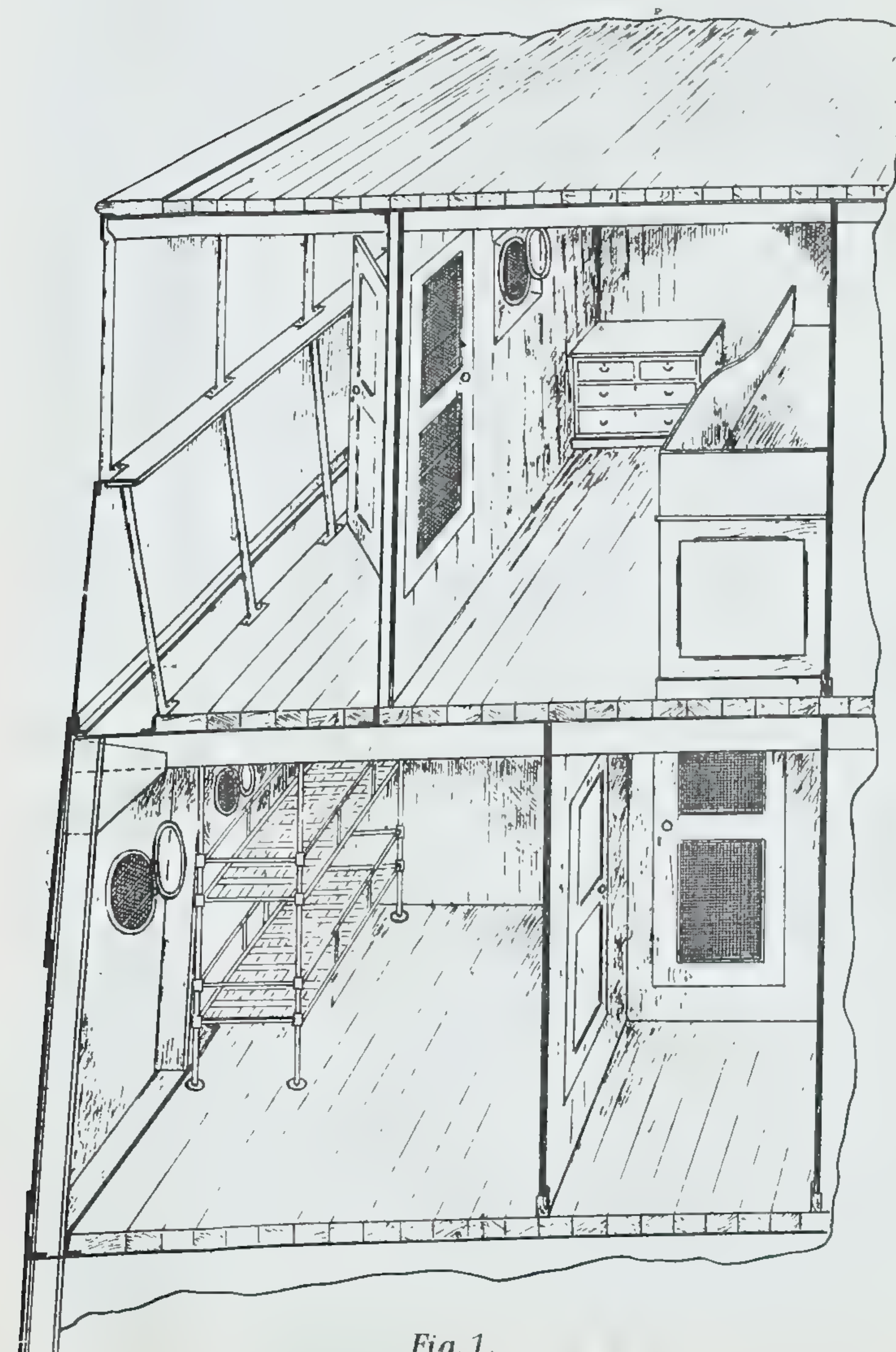

Fig. 1.

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SKETCH SHEWING MOSQUITO PROTECTION FOR DOORS, VENTILATORS AND PORTS.

S.S. "VINCENT."

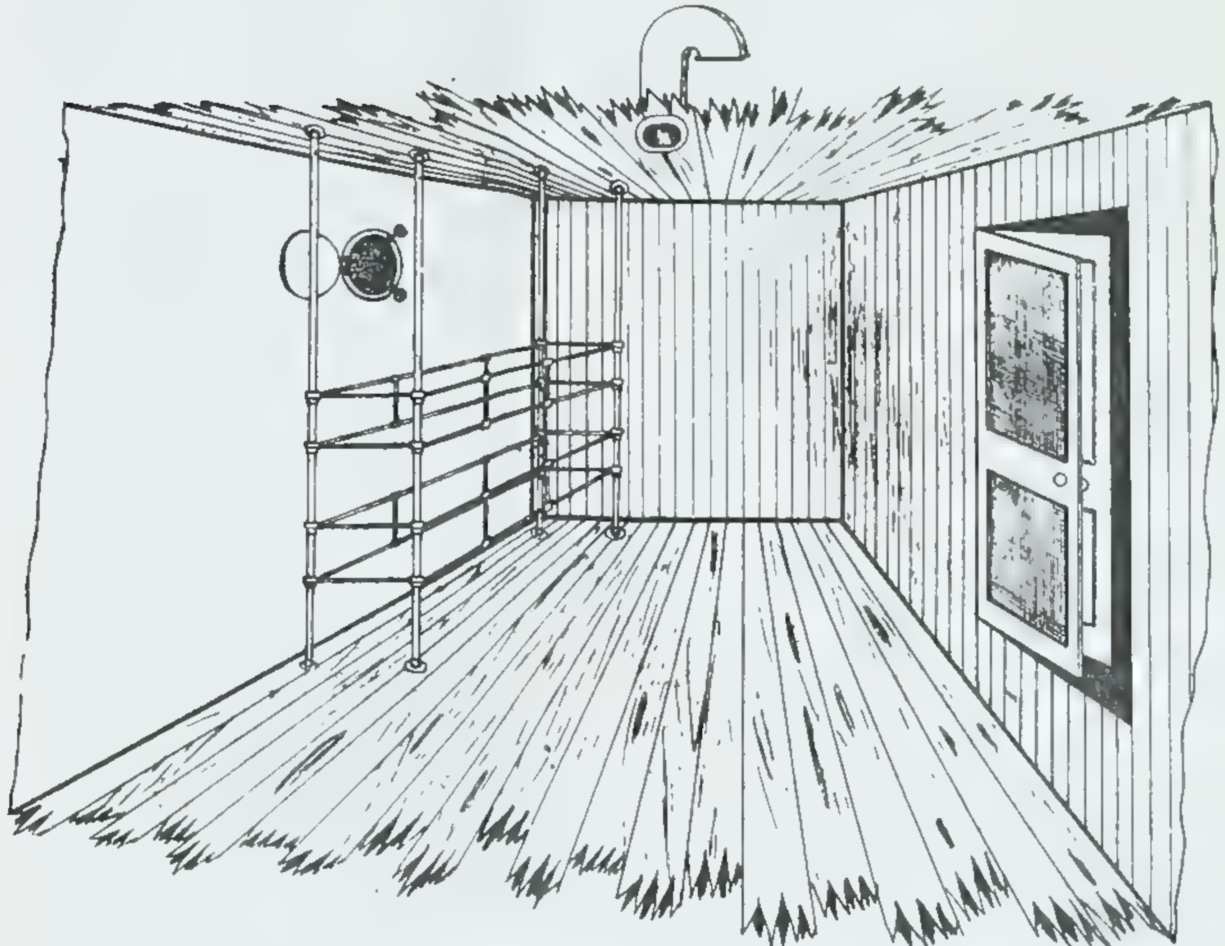

Fig. 2.

DOOR, PORT AND VENTILATOR

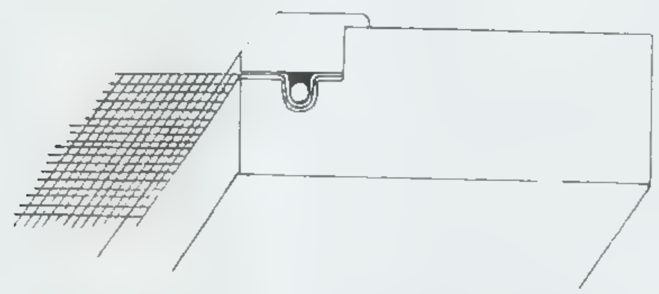

Fig. 3.

METHOD OF ATTACHING WIRE-GAUZE SCREEN TO FRAMES.

Fig. 4.

DOORS AND PORTS.

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BOAT DECK

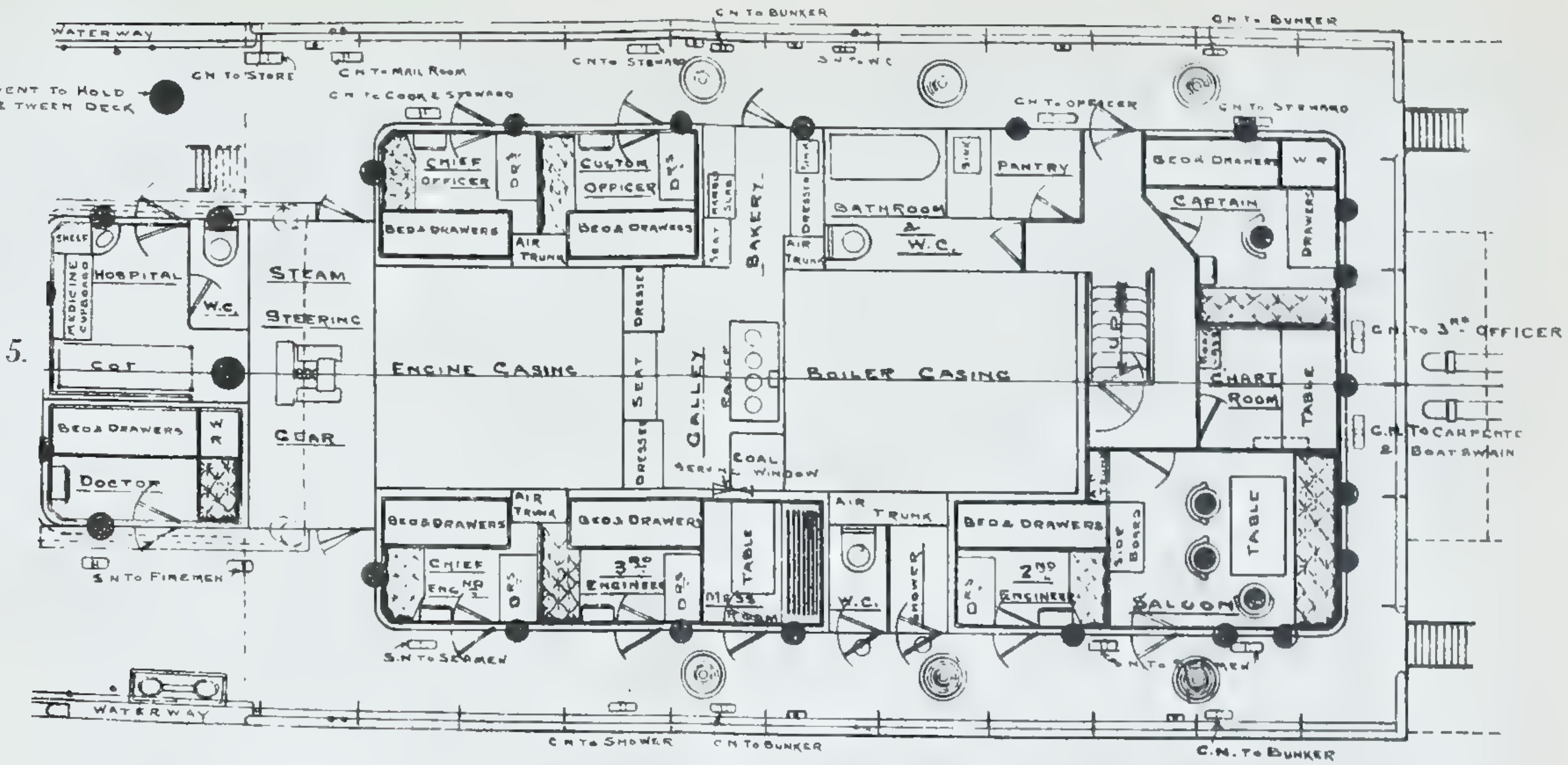


Fig. 5.

POOP & BRIDGE DECK

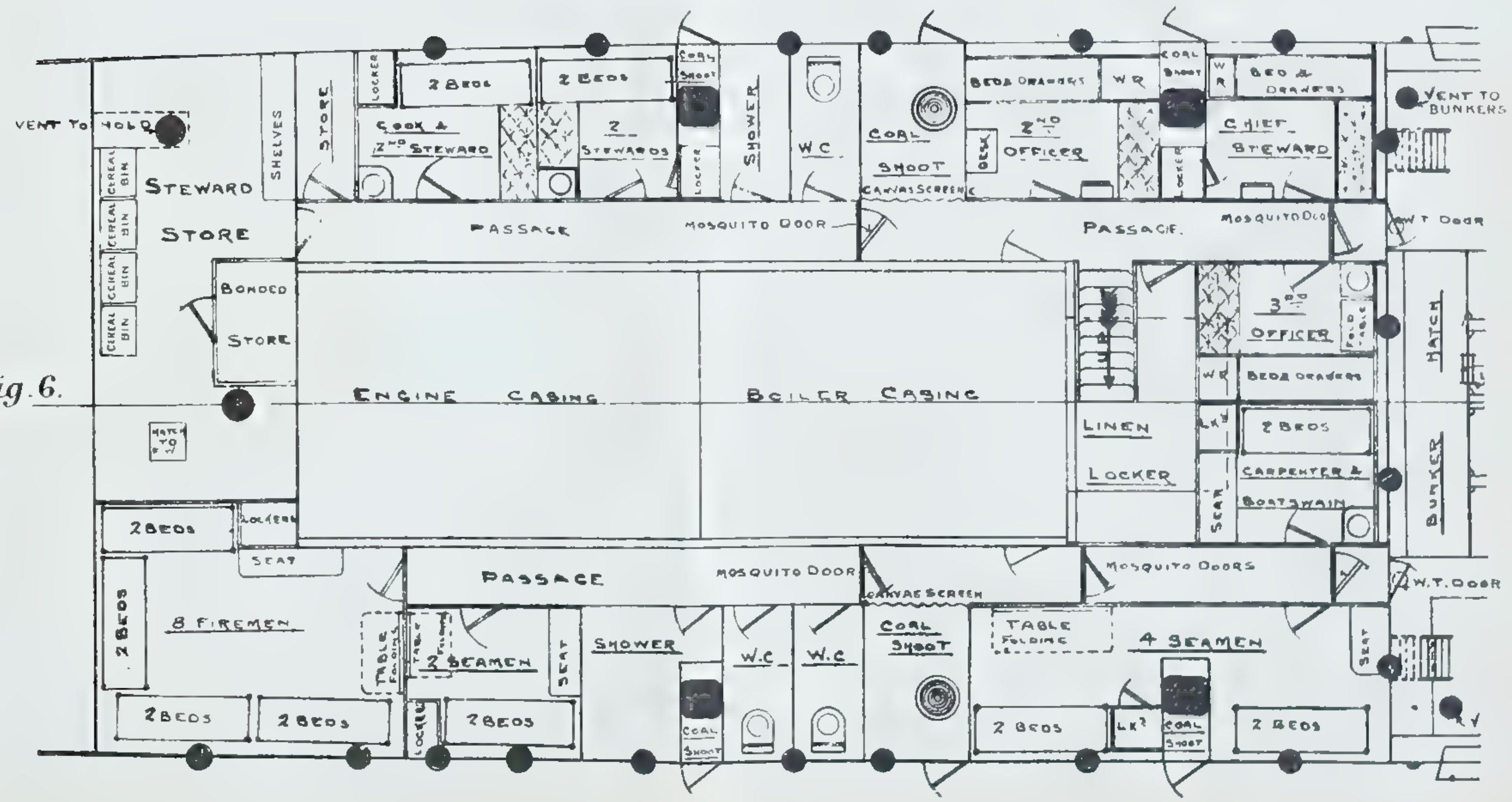


Fig. 6.

MAIN DECK

tropics, and the coal-dust, that would cake and block the meshes of the screen, and thus restrict the inlet of air. The wheel-house is unprotected, as screening would be impossible when navigating a river.

Provided the screening is not disturbed very few mosquitos should be able to gain an entry, and adequate regulations can be made to prevent such acts as the taking off of a port-hole screen or tying back a door.

For port-holes an 18 mesh should suffice, and the gauge of wire need not be thicker than 32 or 30. For doors which are liable to be roughly used, a heavier gauge wire is necessary. The mode of application of the wire-gauze is important. The gauze should not be tacked on to the frame. A far better method is to have a deep groove running around the panel (fig. 3), and a rod either of wood or metal of a size slightly smaller than the groove. The wire-gauze is laid over the entire panel and the rods driven into the grooves. This makes the wire taut and the tension is equally distributed. All strands of the mesh are fastened securely, which is not the case with gauze fastened down by tacks. Over the rod a neat strip of moulding can be fixed, which finishes off the panel and can be easily removed. The great advantage of this method is that it is a comparatively simple matter to take off the strip of moulding, remove the rods, and substitute another piece of gauze. This method is extensively used all through the United States.

Excellent arrangements for large private yachts and even passenger steamers can be devised. I have seen nearly the whole of the bridge deck enclosed with ordinary mosquito-netting. The top was made of tarpaulin stretched on robes, and the sides were made of gauze. At sunset the sides were let down, and from fifteen to thirty people were able to dine and sleep in comfort. The expense is not so great, and is amply compensated for by the relief from mosquitos.[1]

[1] Messrs John Holt & Co. have recently screened an entire ship for West Africa.—R. Ross.

By Dr OSWALDO GONÇALVES CRUZ
Director of the Institute of Manguinhos

47. Prophylaxis of Malaria in Central and Southern Brazil.—Several anti-malarial campaigns have been carried on under the technical direction of our Institute, all of which have been crowned with the most complete success. They were undertaken in the following chronological order:—

(1) In the construction of the Itatinga Railway, Santos.

(2) In the work of damming the Rivers Xerem and Mantiquira, for the water-supply of the city of Rio de Janeiro.

(3) At the time of the extension works of the Brazilian and Northern of Minas Railway.

(4) During the survey work for the Bahia and Espirito Santo Railway.

(5) During the construction of the North West of Brazil Railway.

(6) In the suburbs of the city of Rio de Janeiro in connection with the prophylaxis of yellow fever.

The most important campaigns were conducted by Drs Carlos Chagas, Arthur Neiva and Gomes de Faria, Members of the Institute; and Dr Carlos Chagas (1906-1907-1908) had already written on some of them, he being among us the first to organise a scientific campaign against malaria.

The prophylactic formulae adopted by the Institute comprised the following:—

(1) Prophylaxis by quinine exclusively, adopted when the treatment had to be applied to a moving population

and one not remaining any length of time in a particular zone (railway construction).

(2) Preventive administration of quinine, isolation of the persons infected with gametes, in infirmaries fitted with wire-gauze, and unrelaxing treatment of those infected. This system was adopted when the persons already infected had to reside for some time in the infected zone with healthy persons, and where the work was done more slowly.

(3) The same measures as above, together with systematic sulphur disinfection of infected dwellings. Prophylaxis adopted when the staff remained for a longer period in the infected zone, and where it was not possible to carry out prophylaxis by means of larvicide.

(4) Indirect prophylaxis by war on mosquitos.

Let us see more in detail how these prophylactic systems were carried out among us.

The first prophylactic formula.—This was put into execution with success in the surveys for the South of Bahia Railway, by Drs Mauricio de Abreu and F. Soledade, of the General Board of Public Health. The process consisted in the daily administration of 50 centigrammes of hydrochlorate of quinine. It was compulsory, and the medicine was administered by the doctors themselves, who saw that the patients swallowed the capsule. In this case, as the persons never remained long in the same place, the dose of quinine given was always prophylactic, which is not the case when the persons remain for a long time in the same infected places, as we shall see later. In the special case, the results were most complete; not one of the persons subjected to the *régime* fell sick, whereas others who accompanied the party, without belonging to it, and who would not submit to the same prophylaxis, were attacked by malaria.

The second prophylactic formula.—Here the process followed out included:—

(1) The preventive administration of quinine, in the dose of 50 centigrammes every three days.

(2) The isolation of the persons infected with gametes, in infirmaries protected by wire-gauze during the hours the *Anopheles* sting, being subjected to strict treatment until the gametes disappear.

(3) Early and radical treatment of persons infected for the first time and not themselves infectious (this was done without isolation), in order to prevent them becoming infectious to mosquitos by the formation of gametes.

Dr Neiva, who was entrusted with the carrying out of the quinine prophylaxis on a large scale (among some 3,500 workmen at Xerem), observed that among those treated with quinine cases of first infection began to appear and increase rapidly. He was able to verify these positively by excluding the cases of reappearance noted after the beginning of the campaign. Satisfied that the dose of quinine was not sufficient to guarantee the prophylaxis, he proceeded to administer it at intervals of two days; after some time, fresh cases of first infection began to appear, and these were only really stopped when he commenced to administer the quinine in a daily dose of 50 centigrammes.

It is interesting to note that among the persons thus treated with quinine an outburst of malaria occurred when they discontinued the use of quinine, even though the practice had been continued *after leaving the infectious malarial zone;* and to prevent such attacks it became necessary to continue the use of quinine for a long time, and repeat treatment at intervals. Stranger still, persons who, after leaving the malarial zone, went to reside in places which were certainly not infected, where they continued the use of the quinine (50 centigrammes daily) for a month, as soon as they abandoned this preventive were attacked by malaria. Such persons had remained for fourteen months in the infected zone *without ever suffering any attack of fever.* When under the action of quinine they

had no fever, but harboured plasmodia in the system. Dr Neiva, in view of his observations, admits that during the quinine prophylaxis the plasmodia become gradually accustomed to the quinine and immune *against this poison*, giving rise to a family able to resist the quinine, and similar to the families of trypanosoma which defy atoxyl. Subsequent to this interesting observation of Dr Neiva's (made in October 1907) several other authors have referred to analogous facts.[1]

The occurrence of the new infections could not be attributed to the inefficient administration of the quinine. This was compulsory, and was administered by trustworthy persons, the infections being found to exist in persons who undoubtedly had taken the prescribed doses.

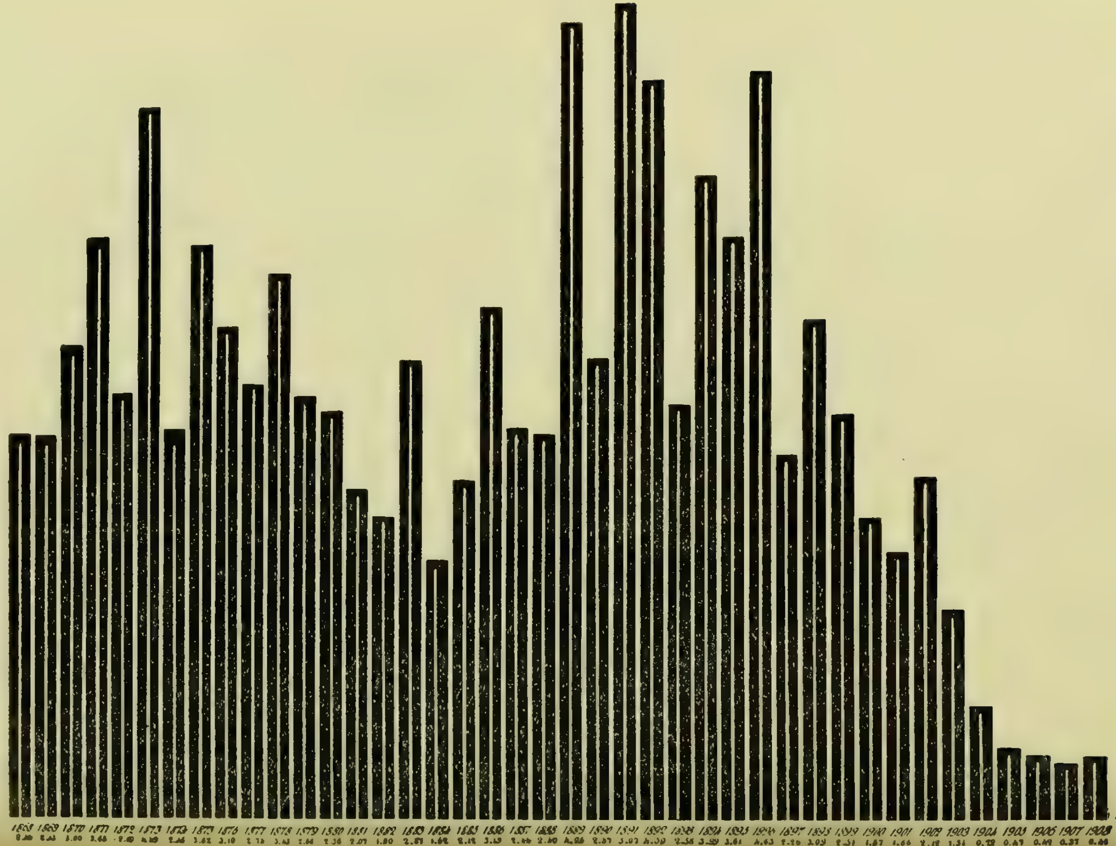
The second item of the prophylactic formula under consideration is intended to destroy the gametes in circulation, which prevents the infection of the mosquitos, which, as we know, can only occur when there are gametes in the periphery. This result was obtained by the daily and continuous administration of 50 centigrammes of quinine. It was proved by the microscope that after a time it was no longer possible to recognise the existence of gametes, whether of the benign or of the malignant (tropical) tertian. When the parthenogenesis of the gametes occurred, following on excessive work or heavy rains, 1 gramme of quinine was given daily, as a rule by intramuscular injection. It should be noted that no scabs appeared after the injections.

The above shows the necessity of carrying on the antimalarial campaigns by persons capable of making the utmost use of the assistance furnished in such cases by the microscope.

The third prophylactic formula.—Dr Carlos Chagas, in his studies on the epidemiology of malaria, and on the biology of the *Anopheles* responsible for the transmission of the trouble within the zones, found that there were dwellings constituting regular hot-beds of malaria, and that such houses were invaded

[1] Incubation retarded by quinine is well known.—R. Ross.

TABLE I.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at night by the *Anopheles*, which stayed there till dawn, some withdrawing to the woods, whence they returned the following night, others remaining concealed in the dark damp places of the huts.

These infected mosquitos caused the permanently infected condition of these dwellings. The opinion that malaria is a disease commonly infectious in houses finds a perfectly feasible basis in arguments relating to the biology of the *Anopheles*, and in numerous facts arising from careful observation. In view of this Dr Chagas resolved to make systematic cleansing by sulphur vapours in such dwellings, and thus succeeded in sterilising these centres of infection.

These measures were always accompanied by quinine prophylaxis. But as an experiment Dr Chagas ascertained that the treatment of dwellings by sulphur was sufficient to prevent the infection of the persons who passed the night in such deadly centres of malaria, and who were not subjected to the action of quinine.

The fourth prophylactic formula.—This consists of indirect war against malaria by the destruction of the larvae of the *Anopheles*. This prophylactic system was tested at Rio de Janeiro, when the prophylaxis of yellow fever was carried out in the metropolis of Brazil. One part of the war on mosquitos was begun by drying up all temporary deposits of water, the destruction of the larvae in natural deposits by means of the cultivation of larvae-devouring fish (*Girardinus caudimaculatus*), by the protection of indoor water-cisterns from mosquitos, the petrolising of large sheets of water, the destruction of the *Bromelias epiphytes*, etc.

The results obtained were very favourable, as may be seen by an examination of Table No. 1, which we give below. In Table No. 2 is given the complete result of the anti-yellow fever campaign. The malaria figures do not show the same decrease as those for yellow fever, because in the table for malaria the cases existing in the urban hospitals, where

patients were received from rural zones not influenced by the prophylactic campaign, appear.

TABLE II.

TABLE OF MORTALITY BY YELLOW FEVER IN RIO DE JANEIRO FROM 1872 TO 1909.

Years.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
1872	1	...	...	...	1	3	1	...	1	8	16	71	102
1873	949	1,168	953	281	180	83	20	3	2	5	2	13	3,659
1874	16	51	168	297	165	69	25	5	4	6	10	13	829
1875	23	168	385	301	244	104	37	7	5	4	3	11	1,292
1876	122	319	1,405	1,019	395	147	41	12	6	4	4	2	3,476
1877	3	10	74	35	40	12	4	15	7	8	18	56	282
1878	156	420	331	130	60	23	18	4	8	10	3	13	1,176
1879	102	227	226	169	81	54	43	26	11	8	9	18	974
1880	138	496	471	273	115	58	18	9	5	5	13	24	1,625
1881	50	67	46	26	25	14	13	6	1	2	4	3	257
1882	3	13	23	27	12	8	1	1	1	...	...	...	89
1883	8	91	335	598	300	111	69	34	12	6	15	29	1,608
1884	79	208	253	210	68	15	12	6	2	...	6	4	863
1885	15	28	58	51	65	57	44	20	18	10	21	58	445
1886	201	351	483	304	74	23	9	2	...	1	...	1	1,449
1887	6	18	89	37	18	8	1	3	2	2	1	13	137
1888	30	39	29	128	116	89	50	18	15	16	49	108	747
1889	510	719	539	142	97	61	27	15	8	8	10	20	2,156
1890	57	103	187	169	109	38	22	6	4	5	9	10	719
1891	51	357	1,026	960	600	421	190	106	62	105	195	383	4,456
1892	1,006	1,290	1,404	410	147	35	14	1	1	1	2	1	4,312
1893	4	57	108	135	172	141	73	48	28	9	13	37	825
1894	371	1,351	1,978	695	305	114	14	12	3	2	...	7	4,852
1895	27	41	86	141	104	77	51	29	23	17	56	166	818
1896	524	731	1,002	445	136	37	20	9	4	4	9	8	2,929
1897	28	33	37	20	17	6	3	...	1	...	1	3	159
1898	22	90	255	287	186	78	65	34	19	13	8	21	1,078
1899	99	170	204	91	48	25	11	10	11	14	14	34	731
1900	42	64	78	61	36	28	9	7	5	6	4	4	344
1901	13	41	69	64	37	17	14	9	10	13	8	4	299
1902	32	64	165	178	154	131	79	50	27	10	18	76	984
1903	133	142	151	99	24	10	9	4	4	2	2	4	584
1904	2	7	7	8	10	4	4	1	1	...	3	1	48
1905	3	13	23	59	64	61	26	9	6	5	8	12	289
1906	6	9	6	8	2	1	2	...	1	3	1	3	42
1907	1	1	6	14	6	4	4	1	1	...	1	...	39
1908	...	...	1	...	...	3	...	...	...	...	...	...	4
1909	...	...	...	...	...	...	...	...	...	...	...	...	0

Table No. 3 shows the results obtained in the campaign undertaken at the time of the damming of the Rivers Xerem and Mantiquira for the water-supply of the city of Rio de Janeiro. The region through which canalisation work had to

be extended was so devastated by malaria that 95% of the men employed in the work were attacked, the mortality being so heavy that the Government was obliged to abandon the first attempt. Work could not be resumed until the special prophylactic service for malaria had been organised, with the result that in a few months the works were completed and the city of Rio de Janeiro provided with an abundant supply of drinking water.

In the malarial zones of Brazil the following species of *Anopheles* are found :—*Cyclolepteron mediopunctatum* (Theob, Lutz), *Cyclolepteron intermedium* (Chagas), *Cellia argirotarsis* (Theob), *Cellia albimana* (Wied), and *Arribalzagaia pseudomaculipes* (Chagas).

These are undoubtedly the transmitters of malaria, as was experimentally shown by the characteristic forms of evolution of the plasmodium in them.

The following species are suspected of transmitting the malaria, so far as can be judged by epidemiological studies :—*Myzomyia lutzi* (Theob), *Cellia braziliensis* (Chagas), *Myzorhynchella lutzi* (Cruz), *Myzorhynchella parva* (Chagas).

As to the habits of the *Anopheles*, Dr Chagas noticed that as a rule they suck only at certain hours, at nightfall, when they come in swarms, or in the early hours of the morning. They disappear entirely during the day and night, at which time, therefore, there is no danger of men being infected by the contaminating mosquitos, or of the mosquitos being infected by sick persons with gametes. An exception to this rule is the *Cellia braziliensis* (Chagas), which attacks for preference during the day and in the sun, which fact shows the necessity of studying the *Anopheles* fauna of the region in which it is proposed to start an anti-malarial campaign.

Further details as to the Brazilian *Anopheles* and their relations with malaria may be found in the works of Dr A. Neiva, lately published (1909).

In dealing with the organisation of the systematic prophy-

laxis against malaria a knowledge of the existence of the *Myzomyia lutzii* (Theob.) in the region is deserving of special attention.

This mosquito, which appears to be responsible for the transmission of malaria in the regions where there are no swamps, lives in the waters found in the *Bromelias epiphytas* (Lutz, 1902), and is so small that it can pass through the wire-gauze of 1·5 mm. mesh, usually employed for protection of houses. For this further reason it is advisable to study the *Anopheles* fauna of the region before proceeding to instal mechanical prophylaxis of malaria.

Finally, it should be mentioned that the quinine prophylaxis has been carried out without the slightest inconvenience to the persons subjected thereto, there being a large number of people who have used daily 50 centigrammes of hydrochlorate of quinine for the last two years and ten months without showing the slightest bad effect from the employment of this medicine. Further, the phenomena of noises in the ears, etc., are not observed when the quinine is administered at meal times. No cases of hoemoglobinuria have been observed following on the administration of the quinine, and the few cases (8) of this morbid nature observed disappeared with the quinine treatment.

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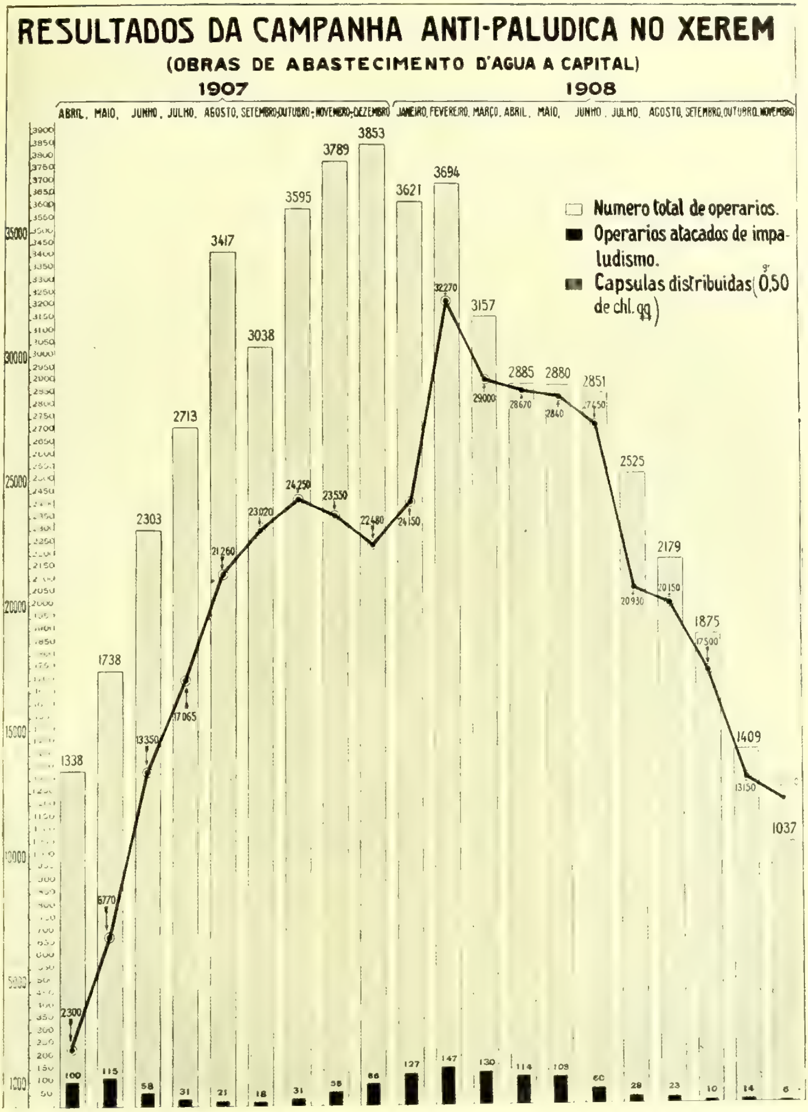
TABLE 3.



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By IAN MACDONALD, M.D., Edin., M.D., Paris

Huelva, Spain

8. Malaria in Spain.—Among the countries of Europe, pain has always suffered severely from malaria, and the ecords of history show that it swept over her with the virulence f plague. In September 1283, when Philip the Bold of France esieged and took Gerona, his army was almost destroyed by ernicious fever, and he himself succumbed to an attack at erpignan. In this epidemic, according to Dormese, great nportance was attached to "some flies which caused death y their bites." Of its ravages in the Peninsula no better icture has been given us than that of Johnson in 1771, "the ttack of Carthagena is yet remembered, where the Spaniards om the ramparts saw their invaders destroyed by hostility of he elements; poisoned by the air and crippled by the dews; here every hour swept away battalions; and in the three ays that passed between the descent and the embarkation alf an army perished." From historical data, it is clear that he greater number of Spanish authors up to the eighteenth nd first half of the nineteenth century in many cases con- ounded malaria with the whole group of infectious diseases. n these times one Spanish clinician, Ludovic Mercado, hysician to Philip II and Philip III, stands apart from the est, for in 1608 he differentiated pernicious intermittent fevers f tertian type in the midst of the pathological chaos around im. In 1783 Masdevall described the lamentable effects of alaria in Lérida and the devastation it caused among the rench troops massed in Cáceres. He pointed out the relation

between fever and the pools left by rivers and after rains, and remarked that the greater number of cases were always found amongst those who worked in the sun and who slept at night in the open air. He completes those accurate observations by recommending the use of Peruvian bark for the cure of the disease.

In the first years of the nineteenth century (1800-1805) several papers appeared on the value of quinine in epidemics of yellow fever, and Nelson in one of his last letters to Lady Hamilton, writes of "the dreadful effects of yellow fever at Gibraltar, and in many other parts of Spain."

Though *Stegomyia fasciata*, the carrier of yellow fever, exists in the cities of Southern Spain, from a study of the symptoms, and course of this fever and the therapeutics employed by these authors, it is clear that they were dealing with severe types of summer autumn fever with hepatic symptoms.

In 1888, discussions began in Spain as to the validity of Laveran's great discovery, and till 1896 the best paper on the parasite was that of Thin and Marshall, who accurately described the types of fever met with in Southern Spain. But the hypotheses of Laveran and Manson, at that date passing from theory to fact in the researches of Major Ross, received no support from these investigators; for their conclusions are thus presented in a well-known text-book (1899): "The observations of Thin and Marshall go to show that in the province of Huelva at all events telluric conditions are the permanent factor in infection, that the plasmodium is not usually conveyed by water, and that there is no evidence of the mosquito being an intermediary host." After the publication of Professor Ross's early papers, in the summer of 1899 we began to investigate malaria in the same province, over an area of 60 miles, extending from the Rio Tinto mines in the foot hills of the Sierra Morena to the coasts around the town of Huelva. In the hills it would be difficult to find a

more arid district, where water appeared to be completely wanting, for the vegetation within a three-mile radius had been destroyed by the fumes of sulphur smoke. It lies 320 metres above the sea-level, and the soil is chiefly ironstone. Hardly any rain falls from May to October (fever season), with the exception of rare thunder-storms.

The aspect of the country is that of undulating hills covered with low brush-wood beyond the smoke area. The valleys in winter are often filled with roaring torrents. In summer these streams dry up, leaving pools, varying in size from about 3 ft. in diameter to the size of a plate or saucer. The district is dotted over with small villages which are situated on the slopes of the hills. The whole area was more or less malarious, but certain villages were notoriously unhealthy, and it was a curious fact that the lowest lying village was the healthiest, though the moving of earth for mining operations went on at its very doors. Yet half a mile off, where most of the British employees lived, fever was frequent. In September 1899, we showed that, so far from mosquitos being scarce owing to the presence of sulphur smoke, they abounded in the district; and in the paludic villages *Anopheles maculipennis* was often the only mosquito to be found in the houses. In the Province of Huelva we differentiated the following varieties of mosquitos: *Anopheles maculipennis*, *Anopheles superpictus* Grassi, *Myzomyia superpictus* Blanchard, *Stegomyia fasciata*, *Culex phytophagus*, *Culex pipiens*, *Culex penicillaris* (*Grathamia penicillaris* Rondani), *Culex spathipalpis* (*Theobaldia spathipalpis*).

In south Spain *A. maculipennis* is the chief carrier of malaria. *A. superpictus* is not often met with; we have only found it in three or four districts.

We have always found that the abundance of *Anopheles* in any district is in relation to the extent of malaria.

Since then, repeated observations in many places in the South of Spain have always shown us the same conditions in fever districts. Whether the town is in the hills or on the

plains, the dried-up water-course is always there, the pools sometimes covered with algae. In the hills the water is often running slowly. In the Sierra we have found *Anopheles* larvae in rocky pools devoid of vegetation, but as a rule they contain algae, at the edges of which the larvae swarm. The difficulty of seeing the larvae of *Anopheles* compared with those of *Culex* is of course well known; and in some cases a careful examination is needed before concluding that there are none in the pool. In the hills, if a muddy pool and a clear one existed side by side, we constantly noted the preference of *Anopheles* for the limpid water. As a general rule we have not found larvae in artificial collections of water — only very occasionally in old barrels and iron tanks on a railway line. In mining districts here, dams are often believed to be a source of malaria. We have not yet found larvae in such collections of water, but have often demonstrated them in the ravines within 50 metres of the dam.

The adult insects in winter and spring hibernate here in stables, hen-houses and pig-styes, where they can always be found until the end of May or the beginning of June. After that time they are also found in the houses. The course of the epidemic season of the malaria in Spain confirms, as elsewhere, the strict relation there is between the human infection of the previous year (winter relapses, cahectics, children) and the infection of the mosquito in June and July, the beginning of the fever season. Our dissections showed that the percentage of the infected mosquitos in June was only 7%, while towards the end of July and in August it rose to 18%.

Of *Anopheles* captured in stables, etc., none were found infected. We frequently found red larvae of *Hydrachnides* fixed on the insects, and sections of some *Anopheles* which died rapidly in captivity were examined by Laveran and were found to contain a fungus in the wall of the stomach and in the coloemic cavity, which possibly may be pathogenic for the insect.

For infection experiments, we fed *A. maculipennis* on fever

cases, and weak hematine gave us good results in staining the zygotes. Sometimes no infection of the insect took place, or the zygotes failed to reach maturity. We felt that such variations were probably due to the diet of the mosquito, and a note by Sehoo of Holland confirmed this; for he pointed out the importance of avoiding acid fruit in the food of insects under infection experiments, and when we fed them on melons the growth of the zygotes was uninterrupted.

The *rôle* then of *Anopheles* in Spain admits of no doubt; and the mosquito has never been found wanting in malarial districts in the fever season when searched for by competent observers.

These investigations were confirmed elsewhere in Spain by Huertas and Mendoza in the province of Cáceres, and by Pittaluga in Catalonia (Barcelona), Valencia, Madrid, and the Balearic Isles, where he found *A. maculipennis*, *A. psendopictus*, *A. superpictus* and *A. bifurcatus.*

In 1903 Pittaluga collected the investigations up to date on malaria in Spain, and from the papers of different writers in that volume some idea was obtained of the extent of malaria in modern Spain. His map gives a good idea of its distribution. Though exact data are difficult to obtain, it is quite apparent that the disease still reigns with great severity in the Peninsula. In 1900 there were 4,703 deaths from malaria in Spain in a population of 18,300,000, which is a malarial mortality of 0·25 *per mille.* In relation to the general mortality of 536,716 (29·3 *per mille*), this represents 1/114 of the general mortality, or 9 per 1,000 of the cases of death. In 1898 and 1899, when the troops came back from Cuba, the malarial mortality rose immediately, and probably gave in each of these years a mortality of not less than 8,000 per annum. The following figures from a railway company give a further idea of the ravages of malaria in Spain to-day. In 1902 12,746 employees were treated medically or surgically, and of these 1,904 were malaria patients. Five deaths were directly attributable to fever,

and the company lost 14,275 days' work in consequence of the disease.

On certain divisions of the line this company keeps a double *personnel* to alternate every fifteen days, and the employees themselves call one notoriously unhealthy area "Little Cuba," where their doctors admit that the mortality is equal to that of tropical countries.

After these researches some stimulus was given to prophylaxis, but, so far, in Spain it has only been undertaken by a few companies and private individuals. In a country where typhus last year raged in her capital, and the isolation hospital is unknown in her provinces, sanitation is only in its infancy. In the city of Badajoz in 1899, in one street 500 people were attacked by malaria, and the supply of quinine ran short in the town. Yet no municipality has yet organised a scheme of prophylaxis.

Through the kindness of Dr Varela we are enabled to submit the following figures from the Madrid, Zaragoza, and Alicante Railway, where protection of the railway servants' houses by wire-gauze, and the administration of quinine, are the methods employed, and have given a gradual but steady diminution in the cases of fever :—

1901-1902	no protection	631 cases
1903-1904	with protection	306 "
1905	" "	190 "

In Rio Tinto, supported by a company which recognises the importance of the health of their staff, we initiated measures of prophylaxis, on a small scale, which have been continued under the direction of Dr Russell Ross, the chief medical officer, with eminently satisfactory results. The English staff of this company, with their wives and families, number about 170 persons. They live in a group of thirty houses, in close proximity to the ravines where *Anopheles* pools are found. A few years ago in the malarial season each house had its fever patient, and the place was notoriously unhealthy. The

pools are now filled up or drained away; and, where it is impossible to remove the water completely, narrow channels are made by which a rapid flow is maintained, and stagnation prevented. Simple open drains have been laid in the bed of some water-courses. The use of nets is recommended, and some families have their houses protected by wire-gauze. The effect on the number of mosquitos has been marked, for now they are hardly ever seen. Hen-roosts and places harbouring domestic animals are smoked out once a week by a mixture producing sulphurous acid gas. The diminution in the cases of malaria may be seen by reference to the following table which we owe to the kindness of Dr Ross:—

		Cases of illness	Cases of acute malaria
1896 . . .	no prophylaxis	89	25
1906-07 . .	prophylaxis	61	2
1907-08 . .	prophylaxis	64	0
1908-09 . .	prophylaxis	78	0
1909-10 . .	prophylaxis	80	1

The attack on the *Anopheles* pools has recently been extended to the ravines round the workmen's villages, where gangs of men under supervision drain away and fill in the pools.

The improvement in the health of the people has been most marked. In former years departments with 2,000 men at times had hundreds incapacitated by fever, and difficulty was sometimes found in maintaining the service. Now, though some cases still occur, fever never rages to the extent it did formerly. Villages which were recognised as hot-beds of malaria, and consequently avoided by the workmen, have been rendered comparatively healthy again. The cost of these measures in Rio Tinto and its environs is less than £100 per annum; but costs are lessened by assistance given by the various departments of the mine. Ten years ago there was a yearly despatch of 1,225 oz. of quinine; it is now reduced to 900 oz., though there is an increase of 50% in the number of workmen.

By Professor ANGELO CELLI

Director of the Institute of Hygiene, University of Rome

49. The Campaign against Malaria in Italy.[1]—In my report to the International Congress, held at Brussels,[2] I described how a Society for the study of malaria in Italy had arisen, and stated that it had initiated its work for the purposes of knowing better and vigorously combating this grave scourge of our country.

I have much pleasure now in noting summarily the results obtained from 1903 till to-day.

Since Ross's discovery was confirmed and consolidated by the work of some of our members, and later by many other workers, we have continued to work in the scientific and practical fields.

In the scientific field we have made researches on the etiology, physio-pathology, epidemiology and prophylaxis of malaria.

In the practical field we have introduced new and improved prophylactic measures into medical use, legislation and the habits of the people.

In order to keep within the narrow limits assigned to me, I can note only the following facts:—

[1] Owing to pressure of work, Professor Celli has asked me to take for his contribution to this book his article in the *Journal of Tropical Medicine* for 1st April 1908 (translated by Dr John J. Eyre), with special additions. The editor of the Journal has kindly permitted this.—R. Ross.

[2] Septime Section. "Hygiene Coloniale, Deuxième Question : Prophylaxie de la Malaria," vol. viii., Bruxelles, 1903, and *Giornale della R. Soc. ital. d'Igiene*, Milan, 1903.

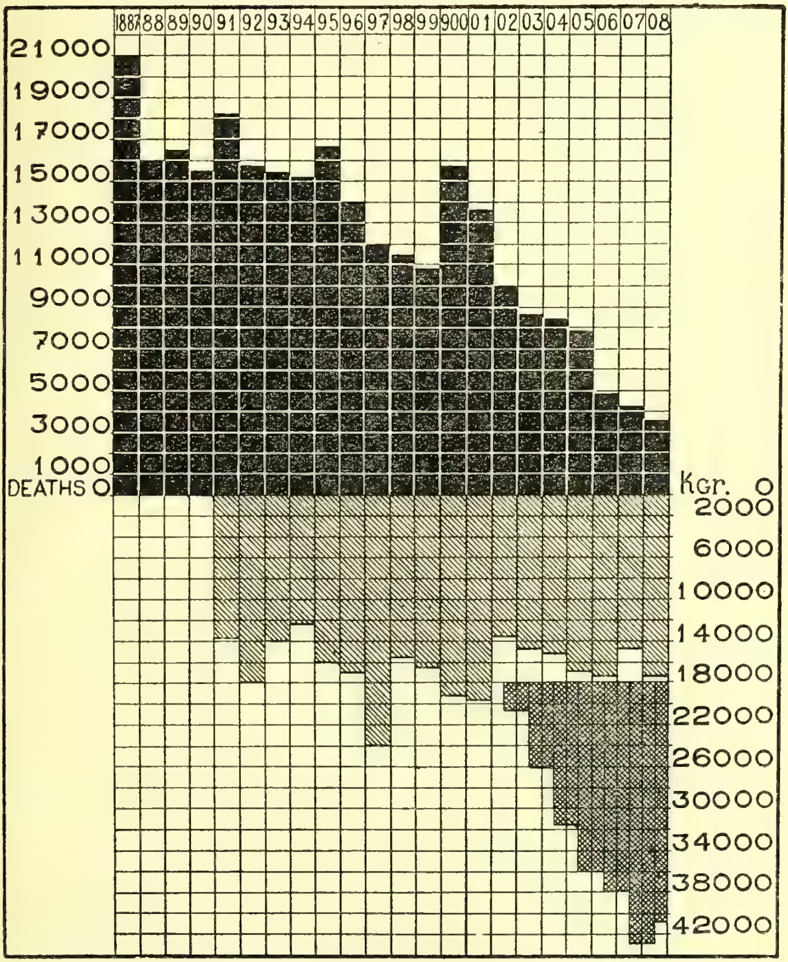

FIG. I.—MORTALITY FROM MALARIA AND THE CONSUMPTION OF QUININE IN ITALY

■ Annual number of deaths from malaria from 1887 (upper part).
▧ Kilograms of quinine imported by private measures from 1891 onwards.
▩ Kilograms of State quinine sold yearly from 1902 (lower part).

I.—THE PRINCIPLES OF EPIDEMIOLOGY

By our researches it has been demonstrated and confirmed in different ways that:—

(1) Man is the only source at present known of malarial infection.

(2) Not only infants, but also children and adults, are the bearers of the infective germs.

(3) The recurrences, sometimes obstinate, in spite of every kind of treatment, are the most marked characteristic of every form of malarial fever. They maintain the contagion between the healthy and the diseased from one epidemic year to the next, and are one of the main causes which regulate the local and annual variations of the malarial epidemics.

(4) As the indication of an epidemy, it is necessary to take into consideration:—

- (*a*) The infantile malarial morbidity and the relative splenic enlargement: this splenic index, as it is named by Christophers and Stephens, may be easily ascertained, but is neither exact nor exclusive; moreover, it does not suffice, when alone, to indicate the intensity of an epidemic.
- (*b*) The morbidity in the whole population or in some classes of it (country people, miners, soldiers, railway employees).
- (*c*) The recurrent cases which are more or less obstinate.
- (*d*) The numerical relation between the æstivo-autumnal and the mild tertian fever cases.
- (*e*) The more or less severe clinical features and complications (haemoglobinuria).
- (*f*) The manifestation of domestic epidemics or of sporadic cases.
- (*g*) The total or absolute mortality related to the whole population (relative mortality), or the one related to

the number of the infected individuals (perniciousness or lethality from malaria).

(*h*) Demographic indications, especially the excess of deaths over births and the following depopulation and desolation which are but too sad a characteristic of malarious lands.

When we take into account all these indications, we may every year not only exactly establish the course of epidemics in our districts, but also precisely compare our observations with the international ones.

An historical description of epidemics in these last years, by making use of the above-mentioned indications, would be possible only for small, well-circumscribed and thoroughly studied areas.

Therefore we must generally content ourselves with the mortality index, and, so far as possible, with the morbidity index too.

(5) The severe tertian, the mild tertian, and the quartan have each a special type of epidemy; that is to say, the first is æstivo-autumnal proper, the second is the only one which presents itself in the spring, the third is principally autumnal.

(6) The *Anopheles* are never wanting where the fevers exist, but their quantity is not always in direct proportion to the intensity of the epidemy; in fact, it is frequently in inverse proportion. On the other hand, there may be plasmodia and *Anopheles* without malaria developing itself, even when malarial patients arrive there from other places, or some autochthonous or sporadic case of fever manifests itself there. Plasmodia and *Anopheles* may therefore persist, and, notwithstanding this, the malaria may become attenuated and disappear.

(7) The number of *Anopheles* infected is always small, even in the places and months most affected by the fevers. The hereditary transmission of the infection from mosquito to mosquito has not been demonstrated up to now.

(8) Various annual epidemic types exist, namely :—

(*a*) The South of Italy type, with great predominance of the æstivo-autumnal parasites, with virulence generally exalted.

(*b*) The North of Italy type, with more or less predominance of the parasites of the mild tertian and the initiation of this in the spring.

(*c*) The North of Europe type, with the absolute predominance of the mild tertian and the precocious development of the epidemy in the spring.

In Hungary, Greece and Russia epidemic types like those observed in Italy are met with.

(9) The mortality from malaria in the whole of Italy from 1887 onwards (*cf.* fig. 1, upper part) has been subject to regular periodic oscillations, with a maximum every five or six years. This periodic course is most marked in the most malarious regions—namely, from Latium downwards, and in the islands (*cf.* fig. 2), while in the rest of Central and North Italy the progressive descent without notable variations is manifest.

The morbidity also from malaria in the Rome hospitals is subject to periodic oscillations which in the last fifteen years have reached their maximum every five years.

No relation has been as yet established between the meteorological conditions and the annual and periodic epidemic oscillations, which would seem to be due more to general biological properties inherent in the specific parasites themselves than to climatic reasons.

(10) In the study of the malarial epidemics, besides infected men and *Anopheles*, it is necessary to take into consideration also other predisposing or immunising causes of biological or organic (x), physical or local (y), economic or social (z) origin. Although they are unknown in their intimate mechanism of action, nevertheless they have undoubted influence both on man and on *Anopheles*, and hence they may stimulate or impede the development of a malarial epidemy. Therefore, for the primary, very simple equation: Malarial man + *Anopheles* = epidemy of

malaria, we must substitute this other: Malarial man + *Anopheles* x, y, z = epidemy of malaria.[1]

(11) The above-mentioned facts show that the problems of

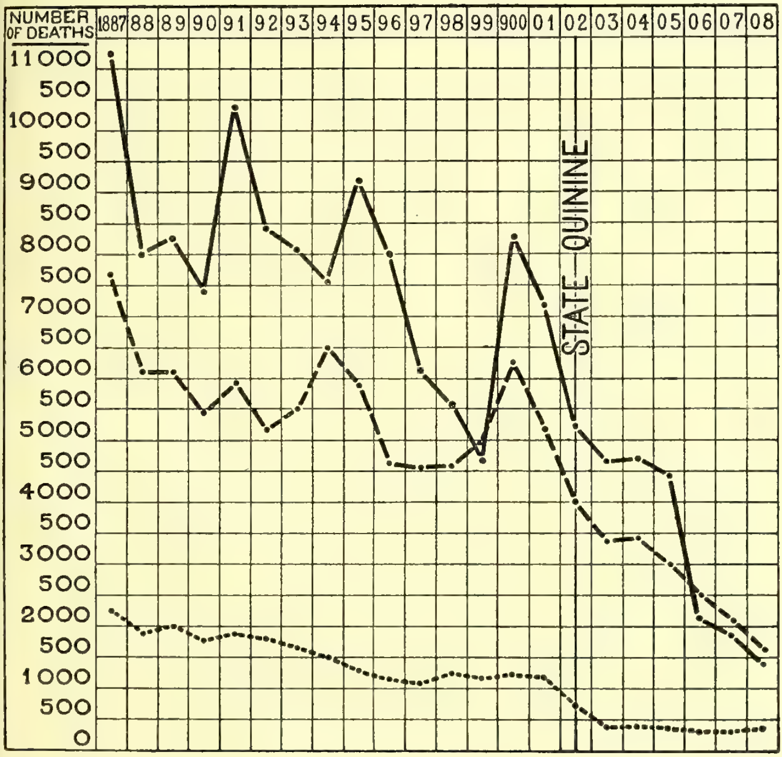

FIG. 2.—MORTALITY FROM MALARIA IN THE REGIONS OF ITALY BEFORE AND AFTER THE STATE QUININE.

——— Latium and South Italy.
\- - - Sicily and Sardinia.
.... Central Italy (excluding Latium and North Italy).

the epidemiology and prophylaxis of malaria cannot be considered from a narrow point of view.

II.—The Measures for Combating Malarial Epidemics

Our first attempts were directed to the *destruction of the mosquitos* by physical and chemical means. The laboratory

[1] Compare section 28.—R. Ross.

experiments were encouraging. Later attempts, with great perseverance, were carried out by Galli-Valerio and by J. Rochaz de Jong, by biological means (animals and plants which kill mosquitos or impede their development). On carrying this into practice on a large scale one meets with many difficulties owing to which, at least among us, with so many extensive marshes and so much intensive culture, one cannot succeed, save in exceptional cases and by long and assiduous work, in destroying malaria by means of the destruction of mosquitos.

Nevertheless, to the old and reliable methods of the campaign against malaria, namely, *draining and agrarian sanitation*, our efforts were directed.

The hygienic effects of the sanitation by drainage already carried out were studied. The result was that on large extensions of land the best drainage sanitation very frequently failed to drain off all the water or to give it sufficient velocity to impede the aquatic life of *Anopheles*. Frequently, also, the deficiency of the neglected state of the complementary drains and of the discharging canals helped to maintain the anophelism.

Consequently, very often the drainage improvement is not synonymous with sanitation, to which one will arrive in time, when the agrarian sanitation supervenes, which, therefore, must be the necessary complement of the drainage improvement.

Both, however, as we shall see, can, and should, be facilitated and shortened — with incalculable saving in health and time —by means of the medical, mixed, or at least the quinine prophylaxis.

In its turn the agrarian sanitation helps to improve the conditions of life of the worker by better housing, alimentation, work and clothing. These coefficients, together with improved medical assistance and the consumption of quinine, have led after a long time, and now lead more quickly, to the sanitation of a territory, even where paludism, with the relative anophelism, still exists, and even can *usefully exist*, inasmuch as it helps to render, by the utilisation of the waters, the agriculture more prosperous.

Keeping away infected mosquitos from the person and from he house is another mode of protecting oneself from malaria, vhich is directly derived from the new theory.

However, if man on whom this scourge weighs should wish o free himself from it only by the mode which is directly erived from the theory of the *Anopheles*, that is, by the destruc-ion, or at least by the keeping away of these insects, who nows how long he would have to wait?

We refer to the difficulties met with by us in our *campaign* 1 endeavouring to destroy the mosquitos.

A quicker and, *within certain limits, a more easy method is prevent them biting one by keeping them away. But only the iinor part of the population, namely, the richer and, therefore, the ast attacked, can enjoy this benefit.*

In fact, in order to prevent the bites of mosquitos one equires, above all, *proper clothing*, which frequently is wanting, nd owing to the great heat cannot be supported; thus, also, ne gloves and masks for protecting the uncovered parts of the ody are very inconvenient, and prevent one working.

Odours, ointments, and washes containing *culicidal sub-tances*, which, according to our researches and those of others, re many, have a very restricted action in closed spaces, and ttle or no action in the open air.

On the other hand, he who can live in some kind of habita-on, with all the openings protected with wire-netting to revent the entrance of mosquitos, can thus, as a rule, preserve imself from malaria.

This method, which was first adopted by us in 1899, by our dvice is coming more and more into use in the houses along ne railways, as shown from the subjoined table.

This mechanical prophylaxis has been adopted in the 'ustom-house officers' barracks.

It, however, is not equally practicable for soldiers.

Unfortunately, owing to the first cost and that of repair, it

is an expensive prophylaxis; besides, it presupposes a special hygienic education in those who inhabit the protected houses, and that they should retire there during the most dangerous hours of the day, things which are very far removed from the mode of life to which our peasants are accustomed.

TABLE I

Mechanical Prophylaxis along the Adriatic Railways

Year.	Persons protected.	Percentage attacked with fever.		Percentage of controls attacked.	
		Recurrent.	Primary.	Min.	Max.
1899	24	20	20	96	—
1900	27	5·5	7·5	77	92
1901	5,165	3·3	20·2	20	96
1902	5,851	10·1	2·	12	81
1903	8,230	22·5	4·6	10	32
1904	12,378	8·7	2·0	10	27

Fortunately, much easier and quicker results can be obtained, when one wishes it, by the proper administration of quinine.

It is very well known that quinine is a truly sovereign remedy, both for disinfecting the blood of malarial patients and for provoking an artificial medicamental immunity. But this very powerful weapon with a double edge has not been well managed by others; whereas in our hands it has constantly increased in value, not only for the curative treatment, but also for the preventive treatment of malaria.

There is no doubt that this remedy acts in inverse proportion to the degree of development of the malarial parasites in the blood-stream; that is to say, it acts best against the sporozoites directly they are inoculated, and least against the forms destined to maintain the recurrent fevers, and little or not at all against the sexual forms destined to propagate the species. We have thus proved over and over again that some

fevers are pertinacious in recurring in spite of the abundant and protracted use of quinine, either alone or associated with the so-called reconstituents (iron and arsenic). In fact, these latter drugs, under whatever form and in whatsoever way administered, have no value as direct anti-malarial remedies.

It follows from this that even the best treatments *in the pre-epidemic period do not succeed in preventing*, as Koch thought they would, *the development of malaria in the following summer.* Therefore, by the quinine treatment alone to extirpate malaria from an extensive locality is much more difficult than one would *à priori* imagine. In any case, it must be the work of long duration, that is to say, treating in every period of the year, day by day, energetically and assiduously, every case of malarial fever.

Quinine, however, is the best remedy for the radical treatment of malaria, even latent and recurrent. The essential thing is to continue the treatment for a long time with great patience and perseverance; and the secret of success is to *administer the quinine in the most agreeable and therefore also the most tolerable form.* Besides, it is better, when possible, to call in the aid of other means pre-eminently beneficial, such as alimentation, habitation, clothing and moderate work.

Since, therefore, the latent and recurrent infections cannot be prevented, much less eradicated, *it is necessary to do everything to prevent completely the malarial infection itself.*

The quickest and best results regarding the preventive treatment are obtained by *quinine administered for this purpose.*

To establish firmly the scientific and practical basis of this preventive treatment of malaria has been, since 1900, *one of the principal tasks we have undertaken.*

Thus, by degrees, with the assistance and control of able collaborators, we have come to the following conclusions:—

(*a*) That quinine, provided it be *administered daily*, is in average and even therapeutic doses better tolerated, and for a longer time than *à priori* one could have believed; that is to

say, after the first two or three days it no longer produces the least singing in the ears, and is not only completely innocuous, but also acts as an aid to nutrition and as a tonic to the digestive apparatus and muscles, thereby increasing the appetite and the power of work.

(*b*) Quinine taken daily is always present in the blood, and thus prevents instead of producing the phenomena of quininism. Further, there is not, perhaps, another example of a remedy so perfect, nor one which so rapidly establishes itself, and can be prolonged for a long time (up to five or six months), and yet can be interrupted when desirable without any disturbance, and without, although the organism is habituated to the small and average doses, diminishing the curative efficacy of the large doses when they are necessary.

(*c*) But if quinine be given at intervals longer than three days the phenomena of quininism present themselves every time; in consequence the method of intermittent administration, namely, every four days (Ziemann), every five (Plehn), every seven to ten (Koch), although recommended by these able workers, is not preferable to our continuous and daily method.

The administration at intervals of every two or, at the utmost, of every three days (Sergent) may be in some cases employed.

Longer intervals are not to be recommended, owing to the disappearing of quinine from the blood three days after the administration.

(*d*) Intolerance to the salts of quinine insoluble in water, if they be administered in average doses daily, is met with in very few persons, and up till now we have never observed the haemoglobinuria which the intermittent method and the relative large doses have not succeeded in preventing.

(*e*) Intolerance very exceptionally manifests itself if a salt insoluble in water, such as tannate of quinine, be given. This salt is slowly absorbed, especially in the intestine, by the work of the bile and of the pancreatic juice, and it is perhaps for this reason that it has *the value of being generally atoxic*, even in

persons who have a special idiosyncrasy for haemoglobinuria towards the salts soluble in water. By reason of this ordinary atoxicity, as well as the absence of bitter taste, it is specially indicated for young children, and for those adults who show intolerance for the other salts or suffer from malaria complicated by gastro-intestinal disturbances.

(*f*) According to Mariani and Giemsa also, quinine proper that is, in the basic state, is absorbed, and acts very well. Consequently, the solubility or not in water of a preparation of quinine, or its administration with the stomach full or empty, do not in the least deserve that importance which has been and is attributed to them.

(*g*) An essential coefficient of true tolerance *is to administer it in an agreeable form*, hence the forms of comfits or chocolates which we have persistently proposed for the various salts of quinine.

(*h*) He who takes quinine every day, and therefore has always a supply of quinine in the blood-stream, *can undergo with impunity inoculations of blood full of malarial parasites*, and can expose himself with little or no danger to the bites of infected mosquitos.[1]

(*i*) Arsenic and iron do not display any protective antimalarial action, either experimentally or chemically demonstrable.

These are the scientific and practical facts on which we have based the method of preventing new infections, and of reducing the recurrences by means of the daily administration of quinine in average doses, namely:—40 centigrams of the bisulphate, hydrochlorate or bihydrochlorate of quinine for adults and young persons; 20 centigrams of the same salts, or 30 centigrams of tannate of quinine for children; and in every case administered in the agreeable form of comfits or o chocolates. These, especially, owing to the sugar and cocoa have overcome the dread and prejudice against the prophylactic use of quinine.

[1] See end of section 16 and section 65.—R. Ross.

In districts with very severe malaria the above doses are increased to 50 or 60 centigrams for adults and young persons, and one can or must substitute the bisulphates for the bihydrochlorate of quinine. In districts with mild malaria the indicated preventive treatment of children may be given on alternate days.

The efficiency of our method is demonstrated by the following figures, which show the rapidly increasing number of persons who, in virtue of the work of our members from year to year, have benefited by this preventive treatment.

TABLE II

QUININE PROPHYLAXIS IN ITALY

Year.	Number of persons treated for 1 to 5 months with preventive quinine.	Attacked by fevers.				Locality.
		Persons treated.		Percentage of control.		
		Total.	Percentage.	Min.	Max.	
1900	116	52	10·3	63	—	Agro Romano and Pontine Marshes.
1901	538	30	5·5	20	60	" "
1902	3,055	335	7·7	12	81	" "
1903	19,021	932	5·6	38	90	" "
1904	52,690	4,262	8·0	8	80	Various parts of Italy.
1905	59,340	3,458	5·8	9	82	" "
1906	110,804	7,115	6·4	9	50	" "
1907	100,816	4,192	4·1	—	—	" "
1908	102,572	5,780	5·7	—	—	" "

In its turn Table III demonstrates that in the Agro Romano, which has been the birthplace of this prophylaxis, owing to the initiative of our Society, the Red Cross Society and the rural doctors of the communes of Rome, while the number of immunised continually increased from 1900 onwards, the cases of primary fevers and of all malarial ailments treated by the Red Cross diminished. Further, the Rome hospitals

admitted a much smaller number of cases in the years 1902-1904, barely felt the usual quinquennial recrudescence of 1905, and admitted a minimum number of cases in 1906.

TABLE III

QUININE PROPHYLAXIS IN THE AGRO ROMANO

	1900.	1901.	1902.	1903.	1904.	1905.	1906.	1907.	1908.
Prophylaxis in the Agro Romano . . .	79	1,176	3,853	17,506	29,693	38,429	42,726	34,927	33,808
Primary infections treated by the Red Cross Society	1,716 (17)	1,263 (13)	764 (7)	320 (2)	162 (1·34)	250 (1·52)	129 (0·7)	166 (1·44)	127 (1·62)
Malarial patients treated by the Red Cross Society	3,751 (31)	2,366 (26)	2,581 (20)	1,547 (11)	1,406 (10)	839 (5 1)	576 (3·4)	371 (3·2)	437 (2·0)
Malarial patients admitted into the Rome hospitals	6,186	4,725	2,750	2,461	2,991	3,991[1]	2,513	2,486	2,748

[1] Year of periodic epidemic recrudescence

The figures in parentheses are the percentages.

The unanimous agreement of both the doctors and patients during these last years have convinced us more and more that *this daily quinine treatment diminishes notably the recurrence, causes the perniciousness to disappear, prevents cachexia, and very frequently the primary infections, and at least renders the fevers milder and more readily curable by simply increasing the quinine for a few days to the therapeutic dose and then returning to the prophylactic dose.*

Hence it is that from the Agro Romano, where we made our first experiments, the *daily use of quinine, which is as necessary as daily bread in the districts and months of malaria,* has to-day extended to every malarious province of Italy, and it is extending more and more among the rural populations, the State employés living along the railways, and in the Army. Its further extension to those who need it is a question of time, that is, of education and organisation. Inasmuch as, in order to convince oneself of its value, it is sufficient to try it, he who has done so proclaims its value and goes on using it; and thus *by entering gradually into the habits of the country people,* it acquires a wide field of beneficial action, especially among the

very numerous persons who, being without any kind of habitation, are compelled to live and work in insalubrious places.

TABLE IV

The Malaria in the Army

Year.	Number of men.	Total number of cases.	Recurrent cases.	Primary cases.	Observations.
1901 .	189,848	49,94	...	...	
1902 .	199,253	36,52	...	...	
1903 .	206,468	24,14	17,85	6,28	Initiation of quinine prophylaxis
1904 .	210,637	19,21	12,71	6,49	Continuation of quinine prophylaxis
1905 [1] .	204,745	23,00	13,90	9,05	" " "
1906 .	211,718	18,94	12,66	6,30	Quinine prophylaxis becomes extensive
1907 .	202,320	12,46	7,76	4,50	" " "
1908 .	216,679	8,04	5,19	2,85	" " "

[1] Year of periodic epidemic recrudescence.

It is certain that it is much better to be able to live in protected houses, and to make preventive use of quinine. It is precisely this *mechanical and chemical or mixed prophylaxis* which is to-day in use along our railways, and is obligatory for

TABLE V

Malaria along the Adriatic Railways

Year.	Percentage of cases of malaria.	Days of mean duration of cases of malaria.	Mean of days of illness lost every year per person.	Observations.
1888-1905	69·92	7·88	5·48	Without prophylaxis.
1902	44·93	6·99	3·12	Mechanical prophylaxis.
1903	30·32	6·25	1·89	" "
1904	33·10	7·53	2·48	" "
1905	39·44 [1]	7·64	3·01	" "
1906	27·65	7.09	3·39	Mixed prophylaxis.
1907	22·70	8·58	2·41	" "
1908	15·79	9·67	3·17	" "

[1] Years of periodic epidemic recrudescence.

the contractors of public works, and is gradually extending also among the rural administrations.

TABLE VI

MALARIA AMONG THE CUSTOM-HOUSE OFFICERS

Year.	Number of Custom-House officers.	Cases of fever verified.	Percentage.	Observations.
1900-02	1,738	1,035	65·30	Without prophylaxis.
1903	1,751	222	12·73	Mechanical prophylaxis.
1904	1,714	209	12·19	" "
1905	1,721	187	10·86	Mechanical prophylaxis and beginning of quinine prophylaxis.
1906	1,614	118	7·31	Mechanical and chemical prophylaxis.
1907	1,642	73	4·50	" "
1908	1,511	73	4·83	" "

Thus in Italy (where we have employed it from 1899), Algeria and Russia, it has been proved to be the most suitable method for guaranteeing health and the power of work to man even in the most desolate malarious regions.

On consulting Tables V and VI it appears that the mean percentage of the cases of malaria along the Ex-Adriatic railways has diminished from 69·92 to 15·79, and the mean of the days of illness from 5·48 to 3·17, owing to the gradual introduction first of the mechanical and later of the mixed prophylaxis.

Similarly, amongst the Custom-house officers the percentage of malaria has fallen from 65·30 to 4·83%.

From all this one can conclude that *by the triple alliance of the doctor, drainage and agriculture, it is possible to thwart the most formidable enemy of our country.*

It is necessary, however, for the doctor to proceed with the vanguard, systematically protecting man from the fevers and

enabling him, therefore, to live and work well even in the most unhealthy places, and thus break that old vicious circle by means of which man could not live on the land because it was malarious, and it could not have been rendered healthy because man was unable to live on it.

III.—LEGISLATION CONCERNING MALARIA

(*a*) *Laws on State quinine.*—These laws were drafted by us on a very solid experimental basis, and were introduced into our sanitary legislation as soon as their necessity and practicability were demonstrated.

The following laws were promulgated as follows:—That of 23rd December 1900 (State quinine on sale to the public at a minimum price); that of 2nd November 1901 (the right of *workers of every kind* to have gratuitously the State quinine *for the treatment* of the fevers from the communal doctors at the expense of the respective employers); that of 22nd June 1902 (concession at a low price of the State quinine to the communes, charitable institutions, and to those who desire or have to distribute it gratuitously to the workers); that of 25th February 1903, Article 3 (the right of the *poor* to have quinine with other drugs given them gratuitously by the communes or by the charitable institutions); and finally, that of 19th May 1904 (the right of workers of every kind to have it gratuitously also *for prophylaxis*).

These laws, demanded by new social duties, necessarily must have clashed with many private interests.

Parliament had the notable merit of passing them with wonderful solicitude and agreement. But even when the first law was promulgated, we saw that making it was easy enough, but that the difficulty rested in applying it. We therefore had to watch that these laws, which were the synthesis of our experience, were properly applied.

It is to be noted also that every fiscal question having been

set aside, the net profits of the great State administration (see Tables V and VII) went entirely to lessen the causes of malaria.

Further, one can loudly proclaim that our legislation on State quinine *has not only a scientific, but also a moral and social purpose*, inasmuch as it recognises that malaria is a calamity connected with agricultural work; and it wisely imposes upon the employers the duty of preventing the damage by giving the preventive quinine gratuitously to the workers, and of compensating them by giving the curative quinine also gratuitously; and to the relatives of employees in public works it assures, besides, the payment of an indemnity in cases of death from the pernicious fevers.

In our anti-malarial legislation the mechanical prophylaxis is obligatory solely for the State employees, including the railway servants who inhabit the places where malaria is most severe. To the private persons who wish to adopt it for their dependents the State accords premiums, which are deducted from the profits of the sale of the State quinine (see Table VII).

(*b*) *Laws regarding drainage.*—Several times we have urged that our antiquated laws in drainage should be harmonised with the modern, scientific and practical views.

We have succeeded already in introducing new principles of anti-malarial hygiene as a guide in the planning and execution of these works.

Formerly it was believed that the only, or at least the best, mode of combating the fevers was sanitation by drainage, and consequently, in the supreme interests of public health, expense was no object. And, in fact, New Italy has spent or allotted upwards of 500,000 lire for this purpose, which works out at no less than 420 lire (£16, 16s. 8d.) for every hectare (2a. 1r. 35p.) drained.[1] To-day, however, for less cost, namely, with good quinine and healthy houses well defended from mosquitos, we can protect man from malaria.

[1] Compare section 57.—R. Ross.

At the present day we know how to spend much better than in the past these many millions for drainage, which, if well employed, would, without doubt, render the land, at present barren and deserted, definitely prosperous and healthy.

(*c*) *Laws on agrarian sanitation.* — Our first anti-malarial campaign in the valley of the Aniene has clearly shown that reoccupation of many large areas, left uncultivated owing to malaria, can now be easily accomplished; and that the labourers can be kept healthy whilst at work by the application of modern prophylactic and curative measures.

This experiment of ours has inspired several others to adopt the sanitation of the Agro Romano, and has enabled us to encourage and direct, with the sound advice of the medical hygienist, the great undertakings of colonisation arising in different parts of Italy, and destined to become economic successes.

IV.—Contribution to the Organisation of the Campaign against Malaria

We invite, above all, the Government to set the good example of applying the new methods of the anti-malarial prophylaxis to all the workers directly and indirectly depending on it. At present the majority of those living in the midst of malaria reap the benefit of the new laws which protect them from this disease.

Also among the soldiers, sailors and carabineers the new prophylaxis, especially the chemical, has been extending from year to year.

The benefit of the gratuitous quinine has been extending likewise among our emigrants who depart or return suffering from malaria. To many Italians also who live outside Italy in malarial regions it has been, and is, distributed by the Ministry of Foreign Affairs, through the consuls and benefit societies; and to make it better known and appreciated, gifts of quinine

have been offered and accepted by other States which, like Italy, are fighting against malaria.

Directly the above-mentioned law of 19th May 1904 was promulgated, with admirable zeal and disinterestedness our members started what are called *demonstration camps* for quinine prophylaxis; and we have become more and more convinced that nothing is better than these for overcoming the apathy and the prejudices of our people, who, living for ages in malarious places, if they have not proved it, will not believe it possible to protect themselves so easily, or to limit the damage which they are accustomed to suffer, and to believe comes from heaven.

We have likewise made, and continue to make, a wide propaganda of the new theories and laws concerning malaria.

Of manifestoes, handbills, bulletins, reports, extracts from the laws and regulations, our Society has distributed, or helped to distribute, about 2,000,000 copies in all.

For this purpose we make use also of the National Association of Parish Doctors, the *Ordini dei sanitari*, the *Unione Magistrale Italiana*, the *Associazione dei Professori di Cattedre Ambulanti di Agricoltura*, the *Lega Nazionale*, and the various local Leagues of peasants and miners, the *Camere di Lavoro*, and the medical and lay Press.

Public lectures and *conversazioni* also in dialect, repeated explanations and demonstrations, have not been neglected by our members, especially by the doctors and medical students.

Finally, we are indebted to the local anti-malarial committees which we are endeavouring to organise throughout the whole of Italy for an active propaganda by means of facts, publications, lectures, etc.

V.—General Results

The following figures enable us to believe and hope that our labours have not been in vain:—

TABLE VII

STATE QUININE AND MORTALITY FROM MALARIA

Consumption of State quinine.		Mortality from malaria.		Net profits of administration of State quinine in lire.
Financial year.	Kilograms sold.	Solar year.	Total deaths.	
...	...	1895	16,464	...
...	...	1896	14,017	...
...	...	1897	11,947	...
...	...	1898	11,378	...
...	...	1899	10,811	...
...	...	1900	15,865	...
...	...	1901	13,861	...
1902-3	2,242	1902	9,908	34,270
1903-4	7,234	1903	8,513	183,039
1904-5	14,071	1904	8,501	183,382
1905-6	18,712	1905	7,838	293,395
1906-7	20,723	1906	4,871	462,290
1907-8	24,351	1907	4,160	700,062
1908-9	23,635	1908	3,463	769,809

From the foregoing table it appears that the annual consumption of State quinine has progressively increased from 2,242 to about 23,000-24,000 kilograms, and that in these seven years the mortality from malaria has progressively diminished four-fifths.

This intimate relation between the progressive increase of one and the progressive diminution of the other cannot honestly be ignored or denied.

In fact, from 1887 to the end of 1895 upwards of 15,000 persons died annually from malaria. From 1896 to 1902 an attenuation began (see fig. 1), but still 13,000 *in media* died annually, a mortality which was much augmented by that of the period of the recrudescence in 1900. On the contrary, owing to the introduction and the continuously increasing diffusion of the State quinine, the mortality from year to year has rapidly

fallen to less than 4,000 victims, *and the characteristic periodic recrudescences have no longer presented themselves.*

Now, who can deny to-day that quinine is not the sovereign remedy, and that only those die from malaria who do not take it in time and in sufficient quantity? This was, and undoubtedly is, one of the principal causes of the very high mortality in the poorest regions of Italy, because they are the most malarious, and they are the most malarious because they are the poorest.

Well, the lessened mortality from malaria in these regions (see fig. 2) proves to us that the sovereign remedy is used more and more among the poorest, and thus our legislation has begun to reach its high social aim, namely, to break that old vicious circle of poverty and malaria. If one considers, besides, that in the above period the importation of quinine into Italy by private means remained more or less the same (see fig. 1, middle part), consequently the consumption of the "private" quinine has not diminished, while that of the State now equals it, and thus the total consumption of quinine is almost doubled (see fig. 1, middle and lower parts).

Regarding the efficacy of sanitation by drainage, which necessarily proceeds very slowly, there is little to hope. Also to the spontaneous diminution of the epidemy, one can only assign that value which, according to fig. 2, must be calculated on the basis of the preceding years.

It is certain that, owing to the State quinine, which is now *legally* at the disposition of all, no person ought to die from malaria, and many fewer persons ought to contract it.

We remember, however, that from the Maremma downwards, including the *Mezzogiorno* and the islands, very severe malaria has reigned undisputed for centuries; and during the five years that our special legislation has been in vogue, and in spite of the efforts of the administrative powers, there are still many communes which do not furnish quinine in sufficient quantities,

not even for the treatment of the fevers; and for prophylaxis very few workers receive it regularly and sufficiently.

We are, however, at the dawn of the redemption of Italy from this scourge, and in these years of study and struggle[1] our Society, one can say without boasting, has helped to indicate a relatively easy path to the desired goal, and has removed some of the many obstacles to success.

Be this as it may, after having taken a notable part in the international researches into the cause and propagation of malaria; after having studied its epidemy, improved the methods for combating it, and introduced new ones; enriched the patrimony of the pathological knowledge; prepared new laws and endeavoured to improve the old ones; defended the new theories and laws in order to make them more readily enter into the customs of the people, and enlarged the sphere of action for all Italy, we have the serene consciousness of having performed a useful duty towards our country. And we cannot but be delighted that outside Italy, and following our example, other attempts are being carried out, that anti-malarial campaigns are being initiated, and that other societies or leagues similar to ours have been formed in Algiers, Corsica, Greece and Russia, while scientists of all nations visit and appreciate our work. Besides, our anti-malarial legislation has not only been imitated by Austria, and by France for Corsica and Algiers, but has also been wholly adopted by Portugal, Greece, Bulgaria, Argentina. . . . All this enables us to foresee the formation of an *international alliance* against this universal epidemy, as has been formed already against other epidemics.[2]

[1] From 1893 to July 1909, we had at disposal for our work only about 90,000 lire.

[2] On the unanimous proposal of the reporters Ross, Rüge, Galli-Valerio, Savas, and Celli, section vii. of the Fourteenth International Congress of Hygiene and Demography adopted by acclamation the following resolutions:—

"The Hygienic Congress urges upon Governments of malarious countries a more active campaign against malaria by means of all the methods proposed by the reporters to the Congress on this subject, and particularly advises:

"(1) The sale by Government of good and cheap quinine on the basis of the Italian law;

"(2) The appointment of special medical commissioners by the Governments of

VI.—Conclusions

The campaign against malaria should be conducted, two measures acting concurrently:—

(*A*) The one directed (*a*) to destroy, or at least (*b*) render inoffensive, the mosquitos which inoculate man with the parasite.

(*B*) The other directed to destroy (*a*) with specific means, (*b*) with general means, these parasites in the human blood.

(*A—a*) The extinction of *mosquitos* can be obtained by *physical, chemical and biological means.* Frequently, however, in the open country, the extinction is interfered with by the extent to which the reproduction of these insects is assured in Nature; and while, therefore, it requires money and organisation, it can have practical application only in limited districts, and assisted, where possible, by better and more extensive drainage.

On the other hand, sanitation by drainage, while it also requires time and money, which fails in getting rid of all the surface water or in putting it in movement, does not succeed in removing the local conditions necessary to the life of the mosquitos, and even when it can succeed agriculture is frequently opposed to it, as in warm climates the surface water is utilised for the cultivation of rice, etc. Hence it is that sanitation by drainage by itself alone is not always efficacious in eradicating malaria from extensive territories.

(*A—b*) It is relatively more easy in definite circumstances to render the mosquitos inoffensive, even in the most dangerous seasons and places, by keeping them away from persons and houses with both chemical and mechanical means (*mechanical prophylaxis*).

Be this as it may, when one desires to, and can adopt any means whatever directed against the mosquitos, one must never

tropical malarious countries to direct anti-malarial measures and to superintend the collection of the malaria statistics for those Governments."

forget that *in many parts of the world the Anopheles are always present, and nevertheless malaria either has disappeared or has become, or tends to become, attenuated.*

(*B—a*) *The destruction of the malarial parasites* in the human blood, as is well known, can be obtained by means of the *specific remedy, quinine.*

However, it is certain that this acts in inverse proportion to the stage of development of the parasites, and hence it is that the forms destined to assure the conservation and propagation of the species resist most, and that instead the sporozoites resist but slightly. Therefore, more easily and more readily than with the curative use, one obtains the specific internal disinfection by means of the *preventive use of quinine,* and of its preparations soluble or not in water, administered in the most agreeable form, and preferably in average doses daily (40 centigrams *per die*), or in days very near one another, in order to establish a perfect mithridatism.

With this preventive treatment extended in the epidemic period to the greatest possible number of inhabitants of a malarious zone, and with *in every season the most assiduous and prolonged quinine treatment of the malarial patients*, one will see the primary infections immediately reducing themselves to the minimun, the recurrents reducing themselves much, and the pernicious cases and the cachexia disappearing, and thus from year to year the diminution of the epidemy among the respective populations will become more manifest even where the paludal and the local anophelic state remains. But to reach all this on a large scale *it is indispensable that the State itself should manufacture the quinine preparations* in order to sell them *at the minimum price or to distribute them gratuitously*, and always in the *more agreeable form* (comfits or chocolates, etc.).

Hygienic education, with the continuous popular propaganda and anti-malarial sanitary organisation, will do the rest.

Especially, then, with the quinine prophylaxis (eventually coupled with the mechanical) one can immediately, where and

when one desires it, keep man healthy even on very unhealthy land, and thus render possible, or at least more easy and prompt, the drainage and agricultural works necessary for the definite sanitation of even very vast districts.

(*B—b*) Thus more promptly and without the mortality of earlier times we can arrive to-day at the *agrarian sanitation*, and *colonisation of malarious districts*, or establish those *general means*, namely, improving the alimentation, clothing, housing, education of man, which in every period of civilisation augments the organic resistance against the malarial parasites, and by this means more easily subdue malaria, even in the places where anophelism may not become extinct.

Evidently neither of the above-mentioned series of measures for combating malaria exclude one another, but can mutually help one another, and ought to be adopted together in practice.

By CONST. SAVAS

Professor of Hygiene, University of Athens. Physician to H.M. the King of Greece. President of the Greek Anti-Malaria League

I

50. Anti-Malaria Measures in Greece.[1]—Greece is one of those countries which are very severely scourged by malaria. The information, collected by the Anti-Malaria League from all the mayors of the kingdom, and from a great number of physicians, proves that the average number of cases of the disease throughout the kingdom is 29%, or, in other words, out of a total population of 2,631,952 inhabitants, an average of 770,000 are affected by the disease every year. It happens, however, that in some years the number of cases is far heavier. Thus, for example, according to the statistics of the League, in the year 1905 more than half the inhabitants of the country were affected by malaria, and over 6,000 died of the disease.

Of the 69 provinces of the kingdom, 19 show a number of cases equal to over 40% of the number of inhabitants, and of 445 communes only 29 are free from the disease, whilst the remainder are affected in a greater or smaller degree, especially in the cases of 59 communes with a percentage of 41 to 50%, 33 with 51 to 60%, 23 with 61 to 70%, 18 with 71 to 80%, and 2 with 81 to 90%; or 135 communes (nearly one-third of the total number) with a percentage of over 40%. In years of epidemic it happens that in many communes the whole of the inhabitants are attacked by malaria.

The cause of this great frequency of malaria is not only climatological, but is also due to the existence throughout the

[1] Given in English by the author.—R. Ross.

country of numerous marshes and torrent beds. According to the information collected at my request by the Government, there are in Greece 639 marshes, each of an area above one stremma (1,000 square metres), which cover together from 84,657 to 87,903 hectares, or 846-879 square kilometres. As the whole kingdom of Greece covers an area of 60,000 square kilometres, these marshes occupy an extent varying between 1/68 and 1/72 of the whole surface of the kingdom. In addition to these marshes Greece is full of small marshes of under 1,000 square metres in extent, and of torrent beds which owe their existence to the widely-spread deforestation of the mountains, due to repeated forest fires during the long period of Turkish domination, as well as during the Seven Years' War of Independence. These torrent beds are full of small pools which are *par excellence* nurseries of Anophelines, and consequently the chief cause of malaria in Greece.

Malaria is at its height in Greece during the months of July, August and September, and commences to decline in October and November; but relapses continue throughout the winter (from December to March), and with the first heat of May the disease recommences its baleful activity.

Malaria has from prehistoric times been the scourge of Greece. The classical description of the disease in the works of Hippocrates is well known. Information concerning malaria during the decline of ancient Greece is lacking, and it is only after the birth of modern Greece that descriptions of the spread of the disease again appear. Notwithstanding the fact that these descriptions are short and rare, it is to be gathered therefrom that the disease was widely prevalent, especially at the commencement of the establishment of the new state, when the cultivation of the land had not made much progress. However, after the reinstatement of law and order the population increased, villages and towns sprang up, the cultivation of the land spread more and more, and measures began to be taken with the object of draining many of the smaller and some of

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the large marshes, and of regulating the beds of the torrents. By these measures, and at the same time by the application of more careful medical treatment and the wider use of quinine, malaria commenced to decline, and there is no doubt that the disease is now not only less common, but also less virulent than formerly.

This relative amelioration is not, however, due to a systematic campaign against this disease, but to the above-mentioned causes which came about, so to say, automatically with the progress of civilisation. The methodic and systematic fighting of malaria only commenced when, in January 1905, I founded the Anti - Malarial League, which laid the first scientific and practical basis of the campaign against malaria, and met with an extremely favourable reception in all parts of the country. We propose hereinafter to give a concise description of the general measures which have been taken by the Government and by the League, as well as of those special sanitary operations which have been executed either by the Government or by the League or by different companies.

The general measures include the popularisation of the ideas now prevalent regarding the manners in which malaria is caused and repressed, and the undertaking of the supply and sale of quinine by the Government.

II

Propagation of scientific knowledge concerning malaria. — Two years previous to the foundation of the League I issued a pamphlet, *Instructions for Use in Combating Malaria*, which was printed at the cost of the Government and distributed in thousands amongst all the physicians, engineers and mayors of the kingdom. On the establishment of the League, one of its first cares was the publication of a small pamphlet of sixteen pages containing short instructions regarding the

causes and prevention of malaria. Forty-five thousand of these pamphlets have been distributed amongst not only all the local authorities, but also many of the educated inhabitants, especially the schoolmasters, who have been instructed by the Ministry to explain the questions to their pupils. Last year, too, the Government printed 300,000 copies of a leaflet containing the decalogue of malaria. This leaflet was distributed amongst the pupils of the schools, who, after the teachers had explained the instructions to them, were told to give them to their parents. At the same time of late years the daily press and the periodicals of the whole country have repeatedly published illustrated articles on the part of members of the League respecting the causes and prevention of malaria. In addition, some hundred of the leading physicians of the country were invited to Athens by the League. These gentlemen remained for two days in Athens, and consulted with the chief members of the League regarding the ways and means of combating malaria in Greece, and heard us explaining the latest scientific discoveries in this connection. On their return to their homes they acted as apostles of the anti-malaria movement, both amongst the other local medical men, the authorities and the people, visiting the chief malarious districts of the country, and both pointing out the absolute need of the preventive use of quinine, searching out the small pools which serve as nurseries of Anophelines, and applying, wherever possible, the various sanitary measures recommended in the work of Professor Ronald Ross, "Mosquito Brigades," which has been translated into Greek, and printed at Government expense and distributed gratis.

The importance of combating malaria and the manner of so doing have repeatedly been explained to the engineers and heads of the Agricultural Schools, and especially to schoolmasters, whenever they have been assembled for educational purposes by the Government inspectors.

The League has also approached the rich landowners,

furnishing them with printed instructions and pointing out the necessary sanitary measures in connection with the pools on their estates, and urging them to disseminate and apply general measures for the prevention of malaria.

Conjointly with measures for the popularisation of the methods of combating malaria, the League has also attended to the collection of local information concerning the disease. Official Health Statistics are issued in Greece only in the case of the twelve chief towns of the kingdom, the population of which together amounts to 430,000, and they are confined to the number of deaths, whilst with regard to the frequency of disease and the mortality in the rest of the kingdom no official statistics are to be had. We have consequently deemed it right to apply for information to all the physicians of the country regarding the number of cases of malaria in the districts in which they practise. This information refers to the periods of the commencement, the height and the decline of the disease, to the ascertainment of the percentage of cases among the inhabitants, both absolute and relative to past years, and to certain other questions. We succeeded in collecting information of this kind relating to five years. Several of the physicians drew up at the same time lengthy reports concerning malaria in their provinces, and respecting the marshes in the district, with the measures by which the disease may be stamped out. At the request of the League the Government instructed all the local mayors to submit reports as to the number of cases in each commune, and the marshes existing therein. A complete epidemiological chart of the frequency of the malaria in Greece was thus drawn up. This plentiful material was published by the League in three bulky volumes, which also contain the results of a similar work undertaken in the island of Crete by our League. We are of the opinion that the collection of information of this kind is not merely valuable for the sake of the information itself, but also in that it incites physicians, mayors, engineers and people generally

speaking to turn their attention to the spread of the disease and the detriment caused thereby, keeps them in continual intercourse with us and the question always on the *tapis*, undoubtedly resulting in measures being taken with a view to combat the disease.

III

The State Quinine.—In view of the splendid results achieved in Italy by the assumption by the Government of the provision and sale of quinine, the League has recommended a similar measure for Greece. We were also induced to take this view by the fact that considerable abuse took place in connection with the sale of quinine, which is sold in Greece not only by chemists but also by grocers. This quinine not only is sometimes of bad quality, but is often adulterated with foreign substances, besides which it is far from rarely sold underweight to customers, whilst, to crown all, it is sold at a very high price, so as to render the frequent use of quinine by the poorer classes beyond their means. As a means of putting a stop to these abuses the League appealed to the Government and the Chamber, setting forth the harm done by malaria, and at the same time pointing out the means of remedying the evil, the foremost of which is considered to be the undertaking by the Government of the provision and sale of quinine. At the same time the League published the report, which was sent at our request by the Liverpool Tropical School, followed by a draft of a bill concerning quinine, together with an explanatory report, and finally a translation of the Italian laws regarding the combating of malaria.

The Chamber refused to accept the principle of a monopoly of quinine proposed by the League, and on the 15th of December 1907 passed a bill similar to the Italian quinine law. According to this the Government acquires the right to order and to sell any of the salts of quinine recommended

by the Royal Sanitary Council. The order is given by the Minister of Finance on the basis of tenders (except in case the quinine is purchased from foreign State institutions), and fixed upon on each occasion by the Sanitary Council. The sale of quinine is made by the Chemical Laboratory attached to the Ministry of Finance, by the Public Treasuries, post and telegraph offices, educational functionaries, and by other authorities chosen by Royal Decree. The State sells quinine at cost price. It is sold retail to the public by chemists, grocers and other merchants, who derive a small profit therefrom. In the case of bisulphate of quinine the law demands that the retail price per gramme shall not exceed 10 lepta (centimes). The price of other salts of quinine is fixed, as occasion arises, by Royal Decree. The importation and sale of quinine is not prohibited by this law, but such commercial quinine will be examined chemically at the custom houses, and will only be allowed to enter provided the quality is equal or superior to that of the State. Heavy penalties are prescribed for persons selling State quinine or the commercial article at prices above that fixed, adulterating or selling adulterated quinine, smuggling quinine into the kingdom or selling quinine underweight. The law also obliges such communes as are severely scourged by malaria to enter in their budgets an amount destined for the purchase of State quinine to be distributed gratis to the poor of the commune.

After the publication of this law Greece requested the Italian Government to supply the requisite amount of quinine out of that prepared for the latter. The Italian Government was kind enough to consent, and the Greek Government now receives from Italy bisulphate and hydrochlorate of quinine in tablets of 0·20 gramme, packed in boxes containing ten glass tubes, each of five tablets. The contents of the box, together with the price and the above-mentioned penalties, are printed on the outside label. The glass tubes are wrapped in paper containing full instructions for the use of the quinine, in which,

in the case of adults, the dose prescribed is five tablets (1 gramme) per diem for a week, followed by two tablets per diem during the following two months. The inhabitants of marshy districts are recommended as a preventive measure to take two tablets every day from the end of May to the beginning of November. The proper dose for children is also given in these instructions. Besides these tablets the Italian Government supplied phials for hypodermic injections, containing a solution of quinine made up according to the prescription of Gaglio (phials of 1 gramme solution contain 0·40 hydrochlorate of quinine and 0·20 ethyl urethane). These injections have an alkaline reaction, and are painless, whilst they cause neither necrosis nor callosity, and are very easily absorbed. We were also supplied with chocolates of tannate of quinine, each double chocolate containing 0·85 gramme of tannate of quinine, or 0·30 gramme of pure anhydrous quinine (cioccolattini).

This State quinine, which is sold, not only by chemists, but also by merchants of every kind, found its way with astonishing rapidity all over the kingdom. Ten thousand kilogrammes of quinine in tablets and 60,000 boxes of chocolates were sold in the first year after its introduction, which ended in September last. This is a large quantity if it be remembered that the average sale of quinine by merchants during the previous seven years amounted to 6,000 kilogrammes, and that besides this State quinine, a further amount of quinine derived from other sources was sold by retailers. The quantity thus sold is not yet known to us.

The League has especially endeavoured, by means of various publications and by circulars addressed to physicians, to promote as much as possible the preventive use of quinine. We are, however, afraid that the achievement of this object will be difficult, as the majority of people will only take quinine after being attacked by malaria.

We have now given a description of the general measures

employed against malaria, and shall next describe a few of the sanitary measures undertaken in certain districts of the country.

IV

Anti-malaria measures in the plain of Marathon. — After endeavouring by every means to popularise the knowledge of the mode of fighting malaria, the League considered it to be its duty to provide a practical example, by undertaking at least one definite work of the kind, with the object of convincing people of the efficacy of the measures recommended. It was also necessary for the conditions, which are indispensable for the achievement of such work in Greece, to be studied on the spot. With this object in view Marathon was chosen as the field of action.

The plain of Marathon lies on the east coast of Attica, at a distance of 30 kilometres from Athens, and is 11 kilometres in length. It is famous in connection with the battle fought in 490 B.C. between the Greeks and Persians, and contains a few villages, amongst which is that of Marathon, the capital of the commune. The northern part of the plain is traversed by a torrent which runs close to the villages of Marathon and Bey, whilst at the north-east extremity of the plain there are extensive marshes. The following villages and hamlets are situated in this plain:—the village of Marathon with 1,200 inhabitants; that of Bey with 150 inhabitants, at a distance of 1 kilometre from Marathon; that of Lower Souli with 160 inhabitants, at a distance of 5 kilometres; besides these, eight hamlets are scattered over the plain, with 173 inhabitants in all. The total population of the plain thus amounts to some 1,680 souls, most of whom are engaged in agriculture and vine-growing, and a few in poultry and stock farming, and thus spend the greater part of their time in the

plain, returning to their houses to spend the night, with a very few exceptions.

The inhabitants of the plain of Marathon are extremely subject to malaria. An examination, conducted in October 1906, of the pupils of the local school, show that 100% suffered from an enlarged spleen. In May 1907, 1,216 persons were examined, and 1,031, or 84·79%, suffered from fever during the previous summer. These observations coincide with the reports of the Marathon doctors, one of whom put the number of cases of malaria in 1906 at 90%, and the other at 80%. Owing to the great prevalence of malaria the inhabitants, at the commencement of our work of sanitation, were pale and sallow, and their skin dried up, whilst they appeared exhausted and incapable of work owing to frequent attacks of fever. In many cases the spleen was enormous, and reached the pubic symphysis. According to the evidence of the local physicians the mortality was very great, not merely among the children, but also among adults, whilst death occurred not only from pernicious fevers, but to a great extent from pneumonia, which is one of the commonest diseases of the place. Mortality was so common amongst adults that three-fifths of the inhabitants had been married a second time, whilst persons of over sixty years of age were rarely found, whereas longevity is common in the healthy villages of Greece.

Throughout the whole plain of Marathon stagnant waters were found in the bed of the torrent, which flows near the two villages, in the two large marshes, and in small pools between the two marshes.

In this stagnant water we invariably found larvae of Anopheline mosquitos. Both in the bed of the torrent and the houses of the villages of Marathon and Bey the *Anopheles superpictus* was especially noticeable, whilst in the marsh of Lower Souli a large number of larvae of *Anopheles claviger*[1] was found, together with a small number of *bifurcatus*, and

[1] *A. maculipennis* Meig.

in the houses of this village a large number of *Anopheles claviger* was remarked. In the other marsh, which is situated at some distance from the villages, repeated examination brought to light a very small number of the larvae of *Anopheles superpictus* and *claviger*. In all the other distant pools throughout the plain only *Anopheles claviger* was discovered, but in large quantities. The chief source of the malaria in the villages of Marathon and Bey was therefore concluded to be the adjacent torrent, and that of the village of Lower Souli, the neighbouring marsh. The inhabitants of the hamlets in the plain were, of course, infected by mosquitos from the other pools.

The first larvae of *Anopheles* were remarked, in the case of the year 1907, at the end of April, and in 1909 at the commencement of May.

The work of sanitation was carried on at Marathon during three successive years, and only during the summer and the commencement of the autumn. In the year 1907 we commenced on 1st May and continued until November, whilst in 1908 and 1909 we carried on operations from 1st June to 20th October. The staff engaged in this work was composed in 1907 of one physician and one medical student as assistant, both despatched from Athens for the purpose, whilst in 1908 we employed one physician and two medical students, and in 1909 only one physician without any assistant. The physician and his assistants were installed in a small house in the village of Marathon, where they set up a microscopical laboratory, and a pharmacy which included a certain number of absolutely indispensable drugs besides quinine. Patients received advice and treatment gratis. Only sufferers from malarial diseases were admitted, all other applicants being referred to the village physicians.

The work was performed in the following manner:—In the first place, the names of the inhabitants of the villages were entered in a special register, together with their age, and any enlargement of the spleen stated, as well as, in many cases

the result of the microscopical examination of their blood. The inhabitants were next divided into as many sections as there were physicians of the League, and each of these doctors took care of the inhabitants who fell in his section. Patients complaining of attacks of malaria were examined as regards both their spleens and blood, and, if proved to be so suffering, were subjected to the proper treatment as described below. As a means of facilitating our work several lectures were given by our physicians to the people, and by the teachers to their pupils, explaining the plan of the League to render healthy the plain of Marathon, and other related details. At the commencement of our work the peasants showed some distrust. Later on, however, when they were convinced of our good intentions, they willingly agreed to undergo the treatment proposed by us, and showed considerable gratitude at being liberated from the terrible scourge which had from time immemorial tormented their country. We proceeded in the work of sanitation, firstly by destroying the larvae in the torrent bed, and secondly, by administering quinine, which was purposely distributed in a different manner in each of the three years. In order to prevent the development of Anophelines in the torrent bed the water thereof was drawn off in a narrow channel so as to produce a more rapid flow, whilst all pools were treated every ten days with petroleum, which generally is provided by the Government, mixed with tar, at a minimum price for the purpose of destroying locusts. In this part of our work we received valuable aid from the teacher of the village, who indefatigably attended to the carrying out of this measure with the help of his young pupils.

The quinine was given therapeutically throughout the three years, and preventively only during the first and the second year. Sulphate and hydrochlorate of quinine were given therapeutically to patients of ten years of age at the rate of 1 to 1·5 grammes per diem for eight successive days, in the case of those who had suffered one or more attacks, or in whose blood

parasites were detected. Children under ten were treated therapeutically with a quantity of quinine proportionate to their age, *i.e.*, from eight to ten years of age, 0·80 gramme, from six to eight, 0·60, and from four to six, 0·40 gramme. Children under four were given tannate of quinine either in powder or in chocolate; during the first year the sulphate or hydrochlorate of quinine was given in wafers, and during the two remaining years in the shape of tablets.

In addition to the therapeutic employment of quinine during the first year we also used the drug preventively. With this object we gave out quinine to all the inhabitants, irrespective of whether they suffered from fever or not, according to the method of Koch as modified by the Italians; *i.e.*, 1 gramme of quinine per diem for two successive days in the week to those above the age of ten, and a proportionate quantity to the children, *i.e.*, 0·75 gramme in the case of those aged from eight to ten, 0·50 in the case of those aged six to eight, and 0·25 gramme to those of four to six. In the case of infants below the age of four they were given euquinine or chocolate with tannate of quinine. The quinine was given out to the pupils every Thursday and Friday at the school, and every Saturday and Sunday to the other peasants. It must, however, be confessed that this preventive method did not prove to be practically applicable, as the peasants, healthy men in other respects, who worked on week-days in the fields, found such large doses very annoying, owing to the consequent singing in the ears and other discomfort, especially on Sundays, the only day in the week on which they can rest and amuse themselves. These large doses also hindered them in their work during the week. Several of the pupils suffered so much from giddiness and drowsiness after taking quinine that they were unable to attend to their lessons.

Out of the 1,680 inhabitants only 1,544 went through this preventive treatment in 1,252 cases. Unfortunately, all would not continue a regular course, but the figures given hereunder

show the results of the preventive treatment according to the length of application.

Of 67 persons who took quinine during 21-24 weeks none were attacked by malarial fevers.

Of 145 persons who took quinine during 16-20 weeks 30 were attacked, or 20·6%.

Of 220 persons who took quinine during 11-16 weeks 103 were attacked, or 48·6%.

Of 820 persons who took quinine irregularly during 1-10 weeks 464 were attacked, or 56·5%.

It follows, therefore, that out of 1,252 persons taking quinine only 597, or 47·6%, suffered from malaria. It must here be added that in the surrounding villages, which were not included in our sphere of action, malaria was prevalent to a very great extent, so that we may consequently consider that, without our intervention, we should have had the usual number of cases of malaria at Marathon too, or from 80 to 90%, if not more.

An investigation carried out in May 1907 showed 57% of enlarged spleen out of 125 pupils of either sex, and of 39% out of 752 adult inhabitants. After the conclusion of the sanitary work, or at the end of October, an examination of 110 pupils showed only 35% with enlarged spleens, and of 509 adults, 25%. The proportion of cases of enlarged spleen was therefore less at the end of the summer season, thanks to the sanitary measures, without which, it will be easily understood, it would certainly have been greater. In the whole district under our control one person died of pernicious fever and three persons of blackwater fever. All three were treated by the local physicians. In 1905, according to information furnished by the doctors, seven persons died of malarial diseases.

During the summer of 1908 we changed the manner of giving out quinine, and adopted the Italian preventive system; *i.e.*, between 1st June and 20th October we gave out every day two tablets (0·40 gramme) in the case of persons above the age of ten, and one only (0·20 gramme) in that of children below

the age of ten down to the age when swallowing the tablets was not possible. In the case of infants unable to swallow the tablets we gave chocolate with tannate of quinine, as furnished by the Italian Government. As, however, we only had a small stock of these, we confined their use to the cases of twenty-six infants, and for a short period only. It may therefore be said that the preventive method was not applied in the case of infants of under three years.

Out of 1,306 inhabitants of the villages of Marathon and Bey only fifty-six (4·28%) suffered from malaria during the whole summer. If, however, we deduct from these two pregnant women, who took no quinine from fear of causing a miscarriage, twenty-one children, who only took quinine irregularly owing to the lack of "cioccolattini," one who came infected from Athens, one who suffered from urticaria owing to the use of quinine, and one of the local doctors, who refused to undergo the preventive treatment, together with four persons who were infected at the commencement of our work, there remain only twenty-six cases of malaria, and the percentage is reduced to 1·99%.

It is to be remarked that in the surrounding villages the malaria was very severe.

There died only one child of those who regularly took quinine, of a disease which the local doctor who was attending it described as pernicious fever, without, however, parasites being discovered in either of two examinations which were made of the blood.

With regard to the examination of the spleen of the pupils this showed an enlarged spleen in 31 to 43% at the beginning of June, and 7·40% at the end of October.

This method of giving out quinine was welcomed by the inhabitants, who both remarked the beneficent results and experienced no ill-effects, and showed great willingness in adopting it.

During the third and last year of our work at Marathon (the summer of 1909) we neither took measures against the

larvae of the *Anopheles*, neither did we give out quinine for preventive treatment, but confined ourselves to the therapeutic employment of the drug, according to the Austrian treatment adopted in Dalmatia. We therefore gave out quinine only to those who had suffered distinct attacks of malaria, and whose blood contained the parasite. The quinine was given out in the following way:—one gramme of State quinine every day for a week, followed by 0·40 gramme every day for two months, to patients of the age of ten and above. In the case of children from eight to ten years of age, 0·80 gramme every day; from six to eight, 0·60 gramme; from four to six, 0·40 gramme in each case for a week, followed by 0·20 gramme every day for two months. In the case of children below the age of four years State chocolate with tannate of quinine was given. The result of this treatment was, that out of the 1,312 inhabitants only 176, or 13·41%, suffered from malarial attacks, attested by microscopic examination of the blood. The fifteen infants which were treated therapeutically with cioccolattini only suffered a single attack in each case.

Examination of the spleen after the termination of the sanitary work showed enlargement in 14·48%, while at the commencement of January it amounted to 2·67%.

During the whole summer two children died of malignant fever, according to the diagnosis of the local doctors, which, however, was conducted without a microscopical examination of the blood.

The quantity of quinine expended in these three sanitary campaigns at Marathon amounted to an average of 15·6 grammes for each inhabitant in 1907, 32·3 grammes in 1908, and 5·6 grammes in 1909.

The total expense[1] during 1907 reached Drachmas (= Francs) 5,715—or an average of Drs. 3·70 for each patient undergoing the preventive treatment. In 1908 the amount expended reached Drs. 6,736—or Drs. 5·15 per head; whilst in the year

[1] Including pay of the medical men.

1909 Drs. 2,226 were spent, or an equivalent of Drs. 1·69 for each of the inhabitants. It consequently appears that the Italian system is more expensive than the other methods.

We gather the following conclusions from our three years' work at Marathon. The Koch preventive method seems likely to meet with considerable difficulties in our country; that of Celli, in combination with the destruction of the larvae of the *Anopheles*, gives excellent results, and is an ideal method of combatting malaria. Nevertheless, that method is not easily applicable in every part of the country, as it requires continual superintendence of an expert in order to oblige the inhabitants to continue taking quinine every day during the whole summer, and for a series of years, until a permanent cure is effected. It can, however, be employed with excellent results in the army and the navy, in the prisons, railway and other companies, in mines, in villages forming private property, and, generally speaking, everywhere where there is a single authority, with the power of enforcing that method willy-nilly upon those under it. Throughout the rest of the country, however, which is inhabited by independent people, who regulate at will their life and diet, it is difficult for the Italian method to be introduced. Those, however, who have suffered from fever and have taken quinine at irregular intervals without being able to throw off the disease, will be gladly surprised to see the favourable results which are produced by the methodical use of quinine such as applied by us at Marathon during the third year of our work: and so recovering their shaken faith in the drug, they will most probably end by adopting the Italian preventive method.

V

Sanitary work at Laurium.—A work of a similar nature was undertaken during the summer of 1909 by the Greek Laurium Mines Company, the employees of which suffer excessively every summer and autumn from malaria. This work

was confined to the preventive treatment with quinine, as in excavating the mines numerous large hollows are formed in the soil, and these being filled with water and serving as nurseries of *Anopheles*, it becomes impossible, owing to their large extent, to destroy the larvae. Every miner was provided with two tablets (0·40 gramme) of State quinine per diem from the commencement of May to the end of October. The result of this treatment was that out of 480 miners only 28 (5·8%) were attacked by malaria, whereas in each of the foregoing years, 1906-1908, from 30 to 40% suffered from fevers. That this good result is due to the use of quinine may be seen from the fact that, of 75 miners who failed to take quinine regularly, 34 were attacked by malaria.

VI

Sanitary work in Athens.—In the city of Athens the two suburbs which suffer most severely from malaria are those of Pankrati and Batraconesi, where, before the year 1900 the percentage of cases fluctuated between 25 and 30%, and from 1901-1906 it reached 49%, and even as high as 92%, as will be seen in the following table. This excessive prevalence of malaria was due to the small pools in the bed of the river Ilissos, which were found to be full of larvae of *Anopheles superpictus*.

As a means of coping with this situation, the League requested the Government to undertake certain sanitary works in the bed of the Ilissos, and, as a matter of fact, in the summer of 1906 the Government carried out the work in question, which consisted chiefly in levelling the inequalities in the river-bed, and in drawing off the water by means of a narrow channel where the development of *Anopheles* was hindered by the rapid flow of the water. In 1906 the work was but slowly conducted, and consequently the influence upon malaria was slight, as shown by the table given below. During

the second year, 1907, the percentage of cases of malaria in the Pankrati fell to 2 or 3%, and in the Batraconesi, where the work was carried out later in the season, it fluctuated between 25 and 30%. In the summer of 1908 the work of sanitation commenced at the proper time in the Ilissos, and as a result the number of cases of malaria fell to 1%. In 1909 the work was also carried out in due time and the cases fell to 0·66%. It is to be remarked that no preventive use of quinine was resorted to, and so the satisfactory results are entirely due to the work carried out in the bed of the Ilissos.

COMPARATIVE TABLE OF THE CASES OF MALARIA AMONG THE CHILDREN IN THE PANKRATI AND BATRACONESI SUBURBS

Before the adoption of the sanitary measures (*Pezopoulos and Cardamatis*)

					Percentage
In the summer of 1901 out of	280	children	260	were attacked .	92·85
" " " " 1902 " "	200	"	160	" " .	80·00
" " " " 1903 " "	235	"	192	" " .	81·70
" " " " 1904 " "	180	"	89	" " .	49·09
" " " " 1905 " "	200	"	185	" " .	92·50
During the five years " "	1,095	"	886	" " .	80·90

After the adoption of the sanitary measures (*Cardamatis*)

					Percentage
In the summer of 1906 out of	301	children	177	were attacked, .	58·80
" " " " 1907 " "	345	"	73	" " .	21·15
" " " " 1908 " "	300	"	8	" " .	2·66
" " " " 1909 " "	300	"	2	" " .	0·66

BANKS OF THE ILISSOS, ATHENS, UNTRAINED. BY DR CARDAMATIS.

BANKS OF THE ILISSOS, TRAINED. BY DR. CARDAMATIS.

[*To face page* 450.

BY DR PROFESSOR CLAUS SCHILLING

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Abteilungsleiter im Institute für Infektionskrankheiten, Berlin.

51. The Prevention of Malaria in German Possessions.—When Germany began colonising, she found herself face to face with the problem of developing the raw material of the territories she had occupied, making it accessible to trade and civilisation. The development arrived at in these twenty-six years has not been such as to render the colonies able to dispense with the support of the mother-country. The empire has also to make it her care to improve the health conditions of those under her protection. But the science of Hygiene is able to obtain only by slow degrees the place due to it in the work of colonisation, conducted on economical principles. It requires the greatest energy on the part of the doctors to obtain what is necessary from the very slender means at the disposal of the authorities.

Of the tropical colonies of Germany—viz.: East Africa, Kamerun, Togo, and the north of South-west Africa, New Guinea, the Carolines, and Samoa in the South Seas—only the two last-named are free from malaria; in all the others it is, next to small-pox, the most widespread of diseases. The following summary gives information as to the *frequency of malaria amongst the natives.*

TABLE I.

	Age in Years.						
	Up to 1.	Up to 2.	Up to 5.	Up to 10.	Up to 15.	Up to 20.	Up to 50.
New Guinea:							
Bogadjim (Koch)	80[1]		41·6	0			
Bongo (Koch)	100		46·1	23·5	0		0
Gazelle peninsula (Dempwolff)	19	32		22	18		12
East Africa:							
Tanga (Panse)	48	88	79	45		15·3	
Dar-es-Salaam: Indians (Ollwig)	29·6	32·5		29·3		16·1	
Dar-es-Salaam: Negroes (Ollwig)	39·0	40·8		20·1		7·6	
Kamerun: Duala (Plehn)	94		92	85		50	
Togo: Anecho (Schilling)	88·8		71·5	74·0	68·0	16·6	

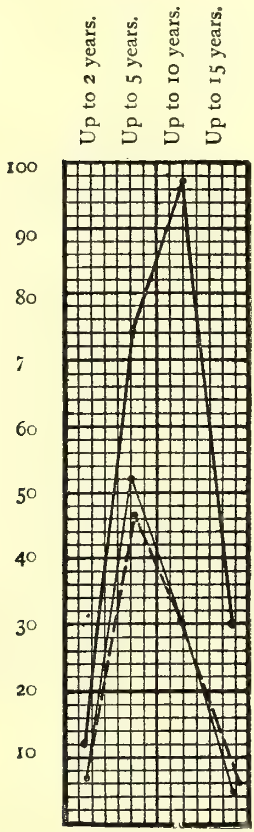
[1] The percentage of those examined found to contain parasites.

It is evident from this table, amongst other things, that the development of immunity in malarial districts is by no means of equal extent. It is evidently connected with the frequency of infection by stings from infected Anophelines. In spots where no decided alternation between the rainy and the dry season takes place (*e.g.*, New Guinea) there is also constant opportunity for fresh infection and for re-infection. This point is to be taken into consideration in a plan of malaria prophylaxis. The table is so far of interest as showing that active immunity in malaria by no means sets in with certainty (*e.g.*, in Duala, only in 50% of the natives who had been exposed at least for ten years to the infection without a break). The chances, therefore, of acquiring active immunity fluctuate between 13:1 and 1:1 during a sojourn of from ten to fifteen years in dangerous malarial districts. The practical unimportance of the objection, that malaria prophylaxis interrupts this active immunity, may be considered as proved by this computation.

Diagrams 1 and 2 show a comparison of the value of microscopic examination of the blood and palpation of the spleen.

They show that in Tanga, East Africa, the two curves of

Diagram 1.



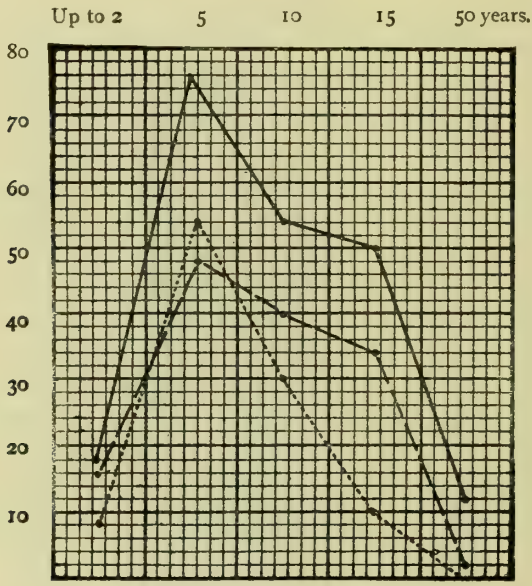
Malaria index in Tanga (Panse).

——— Number of those examined.
— — — Parasites found.
- - - - - - Spleen enlarged.

the spleen rate and the positive results of blood examination correspond approximately, so that the latter could replace the former. But in Anecho it was only in children between two

and five years' old, that palpation of the spleen produced higher results than examination of the blood; whereas in later years the spleen tumour quickly subsides, even where malarial infection exists. So that if the spleen tumour is to be made use of as an indicator of the malarial index of some special locality, the relation between spleen tumour and condi-

DIAGRAM 2.



Malaria index in Anecho (Schilling).

——— Number of those examined.
— — — Parasites found.
- - - - - - Spleen enlarged.

tion of the blood must be first of all ascertained by comparative examination, which must then be taken into consideration in the computation.

To introduce individual therapeutics only on the basis of spleen palpation is not feasible, according to *Panse's* and my own examinations. *Panse*, for instance, found that in a positive condition of the blood the spleen tumour was absent,

7 children under	2 years, in	14% of those examined.				
52	„	„	5	„	28	„ „
31	„	„	10	„	51	„ „
5	„	„	15	„	80	„ „

Without examination of the blood, all these persons would have been deprived of quinine therapeutics.

As regards the *frequency of different forms* of malaria, the following table for Dar-es-Salaam, 1908-1909, gives information.

TABLE II.

There were found	"Rings."[1]	Tertians.	Quartans.	Crescents.
In April-June 1908 . .	1,039	28	122	121
In July-Sept. „ . .	1,184	50	115	173
In Oct.-Dec. „ . .	567	13	87	132
In Jan.-March 1909 . .	594	40	138	165

In reference to *mortality amongst children*, in consequence of malaria, *Steuber* states that before the introduction of prophylaxis in places on the coast, of the total number of deaths amongst children under four years, 44% in Dar-es-Salaam, 57% in Lindi, and 25% in the higher situated Tabora, were due to malaria.

On the introduction of malaria prophylaxis into German colonies, the principle was laid down that it should always be preceded by a statement of the so-called endemic index ; that is to say, by ascertaining what percentage of the inhabitants of one place were infected by malaria at the time. The researches, whenever possible, were extended over various quarters of the town, in order to discover what parts were seriously threatened. In doing this, it turned out, moreover, that not all parts of the population were equally attacked by the disease, or were equally predisposed to it. (See Tables—Indians and Negroes in Dar-es-Salaam.) Further, it is essential that the examinations of the population be repeated at different seasons of the year.

[1] By this are meant the ring forms of all three kinds of parasites, which could not be distinguished one from the other, and were not accompanied by the characteristic forms of division, or by gametes.

It is specially important with children of a year old, whether they have already gone through a rainy, *i.e.*, a malarial period, or not.

In the German colonies the first place is given to *fighting malaria with quinine.* In Stephansort, in New Guinea, *Robert Koch* for the first time adduced positive proof that in a perfect hot-bed of malaria it is possible to reduce this disease to a minimum, nay, make it disappear altogether, and this by the exclusive aid of quinine.

Thus, on the island of Brione (Istria), *Frosch*, under Koch's superintendence, carried on a struggle against malaria in the years 1901 - 1902. At the beginning of the campaign in December 1900 there were 63 = 48% of the inhabitants infected with malaria. In the first nine months of the struggle, 22 cases in all occurred amongst labourers, drawn from regions free from fever; from that time, in the next fourteen months, there were no fresh cases of infection at all, and only nine relapses![1]

The fight against malaria is therefore twofold, consisting, (1) in the treatment of the sick, and (2) in the destruction of the parasites that have already made their way into the human organism—the "prophylaxis" proper. If the solution of these problems can be brought about, malaria must disappear.

Individual prophylaxis can be practised according to an empirical scheme which has proved useful; thus I have found that in the worst fever regions, on marches and expeditions, the dose of one gramme of quinine every fifth and sixth day is sufficient to guard against fever. Other authors recommend other methods; as to the value of the separate schemes, the last word has not been said. But it is certain that individual quinine prophylaxis is able to prevent the outbreak of malaria infection, and, on the other hand, that the use of the drug may be left to the patient himself, under the control of the doctor. It is a different thing with the general part of the fight, with

[1] This work does not belong to my present subject. I mention it here because it prepared for and completed the experiment of Stephansort.

the treatment of the persons infected: the latter can only be detected by the doctor or by a staff specially trained for the purpose, and their treatment must be individualised. The fight against malaria by this method can therefore only be begun and carried out under medical supervision.

In the year 1900 *Koch* found about 734 persons in Stephansort, New Guinea, 157 of whom harboured malaria parasites; 240 Chinese with 63 infected, 209 Malayans with 53 infected, and 264 Milanesians with 157 infected were examined. In the beginning the infected persons were treated with one gramme of quinine, until the parasites had disappeared from the blood; from that date, for two months, every eighth and ninth day one gramme of quinine each was given. The result was a striking one, as appears from the number of patients admitted to the hospital.

January . .	24	admitted	on account	of malaria.
February .	12	"	"	"
March . .	13	"	"	"
April . .	2	"	"	"
May . .	3	"	"	"
June . .	1	"	"	"

Koch emphasises that the months of January to May are notorious as being specially dangerous, and that also in the year 1900 the rainy season lasted right on into June. Nevertheless they succeeded in reducing malaria morbidity to a very slight amount, more especially as the cases of illness from it which had occurred from April to June were only relapses and not fresh cases of infection. And this success was obtained through the rational usage of quinine. It does not make any difference to the importance of this proof, even if one admits that the conditions were rather favourable in Stephansort—being here the case of plantation labourers living far from their own homes and under complete control—and that the success was lost with the time as the control slackened. The proof I mentioned was given, and nothing more has been intended.

It was only natural that *Koch's* experiments in Stephansort were copied in the other German colonies and adapted to local conditions.

In German South-west Africa, the northern tropical districts of which suffer badly from malaria, *Vagedes* has undertaken to make practical use of *Koch's* ideas; because for those districts, where numerous small collections of water such as those which occur in footprints of cattle, horses and wild animals, serve as breeding-places for Anophelines, and where mosquito prevention by means of wire-gauze, screening the native huts, cannot be spoken of, quinine treatment and prophylaxis are the only possible methods. In Franzfontein and the surrounding settlements, *Vagedes* found 30% to 75% of the inhabitants infected with malaria. Every eighth and ninth day, ninth and tenth day, and every ninth, tenth and eleventh day respectively the infected adults were given one gramme at a time; the children correspondingly less. Certainly, it was not unimportant that quinine was given whenever possible in the form of hydrochloride in solution, and only in rare cases in wafers. In children, who easily rejected the solution, subcutaneous administration was adopted. This treatment was continued from two to three months. The results were very satisfactory. In Franzfontein, with its fixed and therefore regularly-treated population, the percentage of infected persons decreased from 79% to 9%; out of 165 persons only 7 new infections and 4 recidives occurred. In Tumamas 57% and in Canas 30% of infected persons were found at the beginning of the campaign. In the course of the following rainy season the settlements were free from malaria.

Koch's ideas of anti-malarial measures have been also applied to Dar-es-Salaam.

Dar-es-Salaam, at the time when *Ollwig* began his activities as director of the anti-malarial brigade, was a town of 12,000 to 15,000 inhabitants. The population consisted of Europeans (officials and officers stationed there for periods of one and

a half years, and merchants with engagements of longer periods), Indians, Goanese, Arabs and Negroes. The Europeans live partly on the peninsula facing north-east, and partly in the districts near the harbour. The Indians live in the adjacent blocks of houses situated more inland, and the native town is grouped around these. The larger portion of the town is surrounded by a depression where water remains for long periods through the dry season, and which renders the cultivation of rice possible. Along the shore wells spring up and form ditches and ponds. The town is therefore surrounded by a ring of water-collections which offer most favourable breeding-places to Anophelines.

The abolition of these water-collections, the land-level being but little higher than the sea-level at high tide, would have involved very great technical difficulty, and consequently would have been most expensive. Other measures, such as oiling of single water-collections with petrol, did not—as was to be expected—affect the number of Anophelines infecting the houses.

Ollwig first tried to establish the malaria index based on microscopic examination of the blood. Palpation of the spleen does not appear to have been practised. *Ollwig* accepts as a sign of malarial infections also the basophile granulation of the red blood corpuscles, if he could find no other cause (anaemia due to ankylostomiasis) for its appearance.[1]

For this purpose the town was divided into twenty blocks, and the inhabitants of these quarters were regularly examined without exceptions. Owing to the fixedness of the Indian and Goanese population, this examination could be fairly well carried out; in the Negro quarters, however, this was an extremely difficult task, as they continually change their domiciles from village to town and *vice versa*. Therefore in the following table the Negroes mentioned have undergone a single examination.

[1] Due to other causes.—R. Ross.

From the table, in the column devoted to "first examinations," the malaria index is given fairly accurately, and shows that spontaneous immunisation in adults does not occur to any appreciable extent, and that therefore the treatment with quinine was justified also in the case of adults.

TABLE III

		Children up to 1 year		Children from 1 to 5 years		Children over 5 years		Adults	
		examined	infected	examined	infected	examined	infected	examined	infected
Indians and Goanese	First examinations	27	8=29%	62	20=32%	181	53=29%	650	105=16
	Later examinations	75	2=2·7%	211	28=13%	415	23=5·5%	1,702	82=4·
Negroes	First examinations	26	9=34%	94	45=47%	288	74=26%	3,338	238=7·
	Later examinations	42	8=19%	169	55=33%	627	99=15%	—	—

The "first examinations" were made in August, September and November 1901, and January to March 1902; the "later examinations" from March 1902 to March 1903—therefore after 1·5 year's campaign at most.

On the contrary, compared to Dar-es-Salaam the malaria index of some villages further inland appeared rather high: of children up to one year of age, 77·5% were infected; of children up to five years of age, 57·3%; of children above five years of age, 31·9%; of adults, 21·7%.

The anti-malarial measures were limited to quinine treatment of all persons who were found infected, as well as those, naturally, who put themselves at the disposal of the doctor.

Ollwig gave adults, at first on three consecutive days and later on every fifth and sixth day, one gramme quinine muriate; after nine days' interval again three quinine days, and so on for two and a half to three months. Children were given a corresponding quantity of the drug in solution and a lump of

sugar afterwards; adults, mostly in form of tablets. One can see it was a question of a purely therapeutic anti-measure, and not one of prophylaxis.

Many of the details in Ollwig's interesting report (one and three-quarter years after the anti-work) cannot be referred to here, and only the "facit" is drawn as resulting from Table III.

In one school (not referred to in the table) they were successful in reducing the number of infected persons from 21·0% (August 1901) to 3·5% (March 1903). Amongst the Indian population from July 1901 to June 1902, 410 cases, from July 1902 to May 1903, 210 cases of malaria, came to the knowledge of the medical officer. It may be noted here that the Indians gradually gained confidence with the medical officer; and so in the first half of the period more infections remained unobserved than in the second half. In the Negro children, on the contrary, the results, though noticeable, were not so favourable (decrease from 34% to 19%). This can easily be understood because the Negro population is subject to constant fluctuations, and it is only seldom possible to continue the after-treatment long enough.

Statistics of adult Negroes have been omitted from Ollwig's report; the 7·6% of infected adults are of little importance in proportion to the 26-40% of the children.

Ollwig proves in his report that the reduction in the malarial morbidity can have been caused neither through climatic factors nor through other possibly adopted measures. The periods of rain and the numbers of Anophelines were not less during the time of the observation than previously, and the work of assainment and petrolisation of the water-collections have been practised so irregularly and to such a slight degree that these measures cannot have had great influence upon the large differences in the numerical value.

Ollwig rightly emphasises that only a very small staff—one medical officer, one nurse, one European and five coloured assistants—has been necessary for this successful struggle.

It is to be regretted that Ollwig did not calculate the total cost, as this calculation would have shown how cheap this method of anti-malarial measures is in comparison to other methods. Up to 1908 the number of native assistants never exceeded half a dozen, besides three to four Europeans, by whom 15,627 natives were examined microscopically and treated with quinine.

Subsequently anti-malarial work was continued in Dar-es-Salaam. For this work only one medical officer was available, whose time was greatly occupied with quarantine and other administrative work; the staff of assistants was not much augmented either. Ross-Ruge's method of blood examination (the so-called thick film method) was adopted, and this method made it possible to discover even quite isolated parasites. The consequence was a remarkable increase in the positive cases since October 1907, and for this reason the numerical values given in Table V, before and after that date, are not to be compared with each other "without further consideration."

Other methods of anti-malarial measures could not be practised except to a very slight degree, as the money provided by the Government was just enough for quininisation. In some houses mosquito-proof rooms—such as verandas—have been arranged; the petrolisation of water-collections could naturally yield but little success.

In *Tanga*, the most important seaport next to Dar-es-Salaam, the anti-malarial work, according to the example of the capital, was not taken up until 1905 (Table IV). Besides one medical officer only three to four nurses understanding microscopical examination of the preparations were available. In addition to microscopical findings, tumours of the spleen decided the need of treatment. This treatment was commenced with one gramme doses in adults, and correspondingly lower doses in children, on two consecutive days per week, and was continued for three months. In all cases an enormous decrease of splenic tumours (from 1,040 to 1,042) and distinct improvement in

the general condition have been stated. Only in very rare cases this cure did not suffice to cause the parasites to disappear.

TABLE IV

			Negroes		Indians	
			Children	Adults.	Children	Adults.
1905-06	All persons with tumour of the spleen or mal. parasites in blood, were treated with quinine for 3 months.	June-Aug. (before quinine treatment)	49·6%	12·4%	54·0%	13·8%
		Jan.-March (after quinine treatment)	24·6%	5·3%	29·5%	3·9%
1906-07	In June, July, and Aug. all nations became quinine prophylactically	May-July	84·0%	14·9%	87·1%	18·3%
		Dec.-March	30·0%	6·1%	31·3%	4·6%
1907-08		1907 April-June	18·3%		12·6%	
		July-Sept.	16·1%		9·5%	
		Oct.-Dec.	14·0%		7·5%	
		1908 Jan.-March	12·0%		6·8%	
1908-09	As the year before	April-June	668	170=24%	2,025	295=14%
		July-Sept.	755	181=24%	1,879	272=18%
		Oct.-Dec.	601	121=20%	2,100	258=14%
		1909 Jan.-March	846	208=24%	2,235	532=23%

The population in Tanga being smaller and much more fixed than in Dar-es-Salaam, the result during the first year was more satisfactory. From March to June 1906, however, the number of infections increased threefold, which is explained by the extraordinary amount of rain during that time. In the course of the following dry season the index sunk to the level

of the previous year. In 1907 to 1909, apparently another rise in the cases of malarial infection occurred in Europeans at Tanga ; but this is due to numerous immigrations from further inland, the people in case of infection having the opportunity to get to the hospital in Tanga by means of the recent railway extension. Among the settled white inhabitants of the city of Tanga the malaria morbidity has gone back from 37% to 35% of the total strength.

Drainage of the town, petrolisation — though not quite general — of water-collections, and deforestation of numerous uncultivated pieces of land, were undertaken simultaneously; also many Europeans' houses were provided with wire-gauze protection.

The increase of malaria during 1908 to 1909 (*vide* Table V) may be due to the hospital being more generally used by the patients from the district, which is more and more frequented by Europeans, and by people employed in the construction of the railway. In Dar-es-Salaam itself malaria has diminished to such a degree among the whites settled there in the European quarter, that individual quinine-prophylaxis is no longer regarded absolutely necessary; rather, numerous officials have passed the whole of their one and a half years' time of service without taking quinine, and without once contracting malaria. Many, however, continue the quinine prophylaxis.

In the other settlements in East Africa, where medical officers and officials are stationed, no proper anti-malaria work is practised to any appreciable degree.

From a general report on infected Europeans who have been treated by the medical officers, it can be concluded that, both together, the anti-malarial measures and the individual prophylaxis in Dar-es-Salaam as well as in Tanga have been a success.

TABLE V

Europeans in Dar-es-Salaam.	1902-03.	1903-04.	1904-05.	1905-06.	1906-07.	1907-08.	1908-09.
Strength, Dar-es-Salaam (*town*), (Iststärke)	311	377	380	657	851	897	743
Morbidity, malaria and blackwater fever in the *district* of Dar-es-Salaam	166	168	141	355	287	268	342
	53·3%	44·5%	37·1%	54·0%	33·7%	29·8%	46·0% [1]
Mortality, malaria and blackwater	7	0	3	4	6	6	3
Europeans in Tanga.							
Strength, Tanga (*town*), (Iststärke)	95	148	192	205	211	222	258
Morbidity, malaria and blackwater fever, in the *district* of Tanga	110	61	178	156	128	206	203
	115·8%	41·2%	87·5%	76·1%	60·6%	92·8%	78·7%
Mortality, malaria and blackwater	2	2	5	6	3	2	2

[1] *Vide* page 464.

The reports on the garrison (Table V*a*) were the most accurate ones, as the majority of recorded infections were observed by a medical officer. The statements about "civil persons" (Table V*b*) are far less precise, only a small number of the malaria infections among them coming to the knowledge of the doctor. The figures showing the morbidity here have therefore been stated far too low, but, as these conditions do not change from year to year, it is quite permissible to compare the different years with each other.

With the coloured population matters are entirely different (Table VI). The percentage value in itself is here not standardised, as it was impossible at the repeated examinations to find always the same persons who had been treated with quinine. In addition to those there were persons who had come into town meanwhile, and who had been treated with quinine only insufficiently or not at all. Some other

TABLE V*a*

Malaria among the European Troops in East Africa.	1893-94.	1894-95.	1895-96.	1896-97.	1897-98.	1898-99.	1899-1900.	1900-01.	1901-02.	1902-03.	1903-04.	1904-05.	1905-06.	1906-07.
Strength	...	123	111	104	112	105	129	130	150	162	130	138	242	189
Cases of malaria . .	...	412	414	343	318	345	386	261	229	209	162	140	252	158
	316%	337%	373%	330%	278%	328%	299%	200%	152%	129%	124%	101%	104%	84%
Deaths due to malaria . .	...	1	...	2	0	1	0	1	1	1	0	0	0	0
Cases of blackwater fever .	...	4	32	19	30	32	19	17	12	8	9	2	6	5
Deaths due to blackwater fever	...	?	7	2	2	2	11	3	1	0	1	1	0	0

comparative instances must be sought, and they are given in the blood examination of the *inhabitants of the suburbs* of

TABLE V*b*

VIEW ON THE MALARIA MORBIDITY IN THE TERRITORY IN EAST AFRICA, AMONG THE EUROPEAN POPULATION.

	White persons.	Civil persons.	Cases of malaria among White.	Civil.	Total.
1903-04	151	1,200	164	292	51·3%
1904-05	151	1,340	171	461	66·1%
1905-06	198	1,688	188	701	47·1%
1906-07	233	2,142	206	500	29·7%
1907-08	192	2,734	156	614	25·3%
1908-09	3,283		892		27·1%

Dar-es-Salaam, who had been examined since October 1907 in sufficient numbers without being given quinine.

The results, concerning children, show clearly how much more favourable the morbidity is among the town children who are partly under quinine treatment. This favourable consequence is not only due to a lesser danger of infection to which the children living in towns are exposed. And this can be seen from the single figures of morbidity which are higher alternatively in the centre and at the periphery of the town. If the danger of infection were less in the centre, its figures of morbidity also should be lower.

Clearly enough, this difference is not as pronounced in the case of adults, in whom acquired immunity plays a considerable part. On consulting Table IV on Tanga, it is quite clear that, under the circumstances prevailing in the seaports of German East Africa, the malaria among the natives can be kept on a certain level simply by methodical treatment of all

TABLE VI

		Special Measures.	Examination of the Blood.				
			Time.	Children. Examined.	Children. Positive.	Adults. Examined.	Adults. Positive.
1905-06	Dar-es-Salaam. Rainfall in the year 1690 mm.	During the rainy season the children of the Indians and natives had been given quinine prophylactically.	Aug.-Sept. (after the rainy season).	610 children.	Of these positive 106 = 16%.		
			Nov.-Dec. (short rainy season).	519 children.	Of these positive 51 = 9·8%.		
1906-07	Dar-es-Salaam. Rainfall in the year 1168 mm.	During the rainy season no quinine prophylaxis amongst the children.	Aug.-Sept.	649 children.	Of these positive 131 = 20%.		
			Nov.-Dec.	564 children.	108 = 17%.		
1907-08	Dar-es-Salaam. 625 mm. rainfall.	Abnormally dry year. Twice a week one gramme of quinine, for three months distributed amongst the infected.	April-June 1907.	825	113 = 13·7%	3,020	103 = 3·4%
			July-Sept.	752	126 = 16·6%	2,452	75 = 3%
			Oct.-Dec.	757	149 = 19·7%	2,793	327 = 11·7%
			Jan.-March 1908.	1,075	269 = 25%	1,389	306 = 22%
	Suburbs of Dar-es-Salaam.	No quinine treatment, so supervision.	Oct.-Dec. 1907.	563	357 = 63%	622	161 = 25·8%
			Jan.-March 1908.	278	129 = 46·4%	945	131 = 13·8%
1908-09	Dar-es-Salaam.	As in the preceding year. Blood preparations according to Ross-Ruge.	April-June 1908.	777	247 = 32%	2,504	996 = 39%
			July-Sept.	919	344 = 37%	3,486	702 = 20%
			Oct.-Dec.	838	173 = 20%	2,763	352 = 13%
			Jan.-March 1909.	676	164 = 24%	3,527	555 = 16%
	Suburbs of Dar-es-Salaam.	No quinine treatment, supervision.	April-June 1908.	206	126 = 61%	898	215 = 23%
			July-Sept.	142	77 = 54%	651	225 = 34%
			Oct.-Dec.	119	73 = 61%	392	113 = 28%
			Jan.-March 1909.	70	46 = 65%	299	85 = 28%

the infected persons. But the statistician, as well as the practitioner in the midst of his work, comes to the conclusion, that a simple diminution does not suffice, and that this diminution is very fluctuating and strongly influenced by incalculable factors. It is, therefore, the keen ambition of the medical officers in Dar-es-Salaam to obtain the means necessary for conducting anti-mosquito measures in the surrounding districts of the town.

At present, engineers are engaged on surveying the town and surrounding districts in order to work out, on the basis of these measures, a plan for the abolition of ditches and other Anopheline breeding-places.

(2). *Kameruns.*—This German territory had, and has partly up to this day, a very bad reputation as a hot-bed of fever. In 1896-1897 the malaria morbidity in Duala, the most important seaport, amounted to about 391% ; in certain months as many as 70% of the Europeans became infected ; about 48% of the Europeans acquired blackwater fever ; and the total mortality caused by tropical diseases altogether amounted to about 8% (Table VII).

TABLE VII

MALARIA AMONG THE EUROPEAN TROOPS IN KAMERUN.

	1900-01.	1901-02.	1902-03.	1903-04.	1904-05.	1905-06.	1906-07.	1907-08.
Strength (Iststärke)	91	85	71	87	84	92	...	89
Cases of malaria	340= 370%	151= 177%	114= 162%	104= 119%	131= 165%	131= 142%	...	87=97%
Deaths due to malaria	...	0	0	0	0	0	...	0
Cases of blackwater fever	20	18	21	23	8	13	...	9
Deaths due to blackwater fever	...	2	2	3	0	0	...	1

Official statistics for the Kameruns are very incomplete, in consequence of the scattered nature of the single stations.

The capital *Duala* is situated on a relatively favourable spot, *i.e.*, on the brink of the rapid Kamerun River, about

6 to 12 metres above its highest level. Here was therefore given the possibility greatly to reduce the number of Anophelines through rainwater conductors and a regular system of drainage of several swampy places. In the latter years Ziemann has worked in connection with the local Government with great energy at the amelioration of the general hygienic conditions. The territory of the town itself has been planned, and cleared of all superfluous bush; and by means of straitly made roads the wind is given free passage. Sewers effect rapid drainage of all rainwaters. A sanitary brigade supervises the accurate and continued adherence to the specified measures. The brigade extends its surveillance also over the surrounding districts; hollows in the neighbouring wood, from where the Anophelines swarmed to the town, were drained or filled up, and if this was not practicable, Saprol was poured over the water surface at regular intervals. Great credit is to be given, however, to the Government medical officers for their work of furthering quinine prophylaxis. It was as early as the close of the last century when *F. & A. Plehn* recommended prophylactic use of quinine, and since then the value of this method, together with general hygienic improvements, has found full recognition with the majority of settlers. Also mosquito-proof houses are recommended and used as an anti-malarial measure, although this method is rendered much more difficult by the humid climate which soon destroys the wire-gauze.

The benefit resulting from the above measures can be easily seen from the figures indicating the malaria morbidity for Duala alone:—

1900-01.	1901-02.	1902-03.
287%	130%	77%

	1902-03.	1903-04.	1904-05.	1905-06.	1906-07.	1907-08.
Number of inhabitants District Duala	142	152	164	183	260	300
Hospital frequented by	187	158	163	151	185	213
Hospital for malaria	83	83	83	68	89	74
Malaria percentage	44·4	53·6	50·9	45·0	48·1	34·7

In spite of the increase of the European population to double its previous strength, the number of severe cases of malaria treated in hospital had, with slight fluctuations, remarkably diminished.

It must be admitted that under such measures also the malaria among the natives is bound to diminish. At the policlinic, quinine is distributed gratis to all natives; the number of those who received quinine in 1907-1908 was 820. The treatment of so many infected people in one place is sure to lower the possibility of infection for the Anophelines and consequently the malarial infections in man.

The number of Europeans in the other settlements in Kamerun is too small, and is also subject to too many changes, to allow the possibility of deducting any conclusions about the value of certain anti-malarial work. The favourable effect of the malaria prophylaxis, and especially the individual quinine administration (prophylactic), together with general improvement in hygienic conditions, is evident anywhere.

The same holds good for the territory of Togo, with its three important places: Lome with about 6,000, and Anecho and Palime with about 3,000 inhabitants each. The two former places are situated on a line of dunes directly at the sea, and landwards spreads a so-called lagoon. During the dry season, from October to April, there is so little malaria in Lome that Dr Krüger (no reference) recommends to suspend the quinine prophylaxis during that time. When, however, in April or May the rainy season commences, the malarial infections soon reappear, for the innumerable little hollows in the bed of the richly vegetated lagoon now fill up, and the Anophelines rapidly increase.

At places with such pronounced "malaria seasons" the quinine prophylaxis must be given first consideration. In Togo it is religiously practised by many Europeans, though not by all, as in 1907-1908 still 99 infected Europeans required medical attendance, 61 of whom, however, contracted the disease "in

to *Koch*, is sufficient to keep the malaria on a low level among the natives in tropical towns.

8. The treatment of the natives with quinine together with individual prophylaxis leads to a remarkable decrease in the malaria morbidity among Europeans.
9. In many cases quininisation alone will not suffice to reduce the morbidity to a degree which is desirable in the politico-economic interest.
10. Which one of these three anti-malarial measures, quininisation, mechanical protection, and anti-mosquito measures, is the most important can only be decided upon according to the local conditions.

PAR LE DR EDMOND SERGENT (ALGER)

52. Campagnes Antipaludiques en Territoire Français.—Le paludisme n'existe plus en France que sur de rares points où il s'éteint d'ailleurs peu à peu : la Vendée, la Sologne, la Camargue.

La culture intensive succédant à de grands travaux d'assainissement agricole, l'accroissement du bien-être et de l'instruction générale peuvent expliquer en partie ce recul du paludisme sur le territoire de la mère-patrie, mais il est difficile d'en préciser rétrospectivement d'une façon parfaite toutes les causes. La survivance des Anophélines au paludisme dans la France entière est un fait très net qui montre que le paludisme peut disparaître sans que son vecteur soit exterminé.

Malheureusement la France, grande puissance coloniale, a dans ses possessions d'au delà les mers un ennemi toujours présent dans le fléau paludéen.

I. *Campagnes antipaludiques en Algérie.*—La plus belle de ces colonies a vu son sort longtemps discuté, non seulement pour des raisons politiques diverses, mais surtout à cause de son insalubrité qui, croyait-on, devait en rendre à jamais le séjour impossible aux races européennes. Alger était occupé en 1830, et en 1841 le Général Duvivier, un héros de la conquête, écrivait la phrase fameuse : "Jusqu'à présent, les cimetières sont les seules colonies toujours croissantes, que l'Algérie présente."[1] C'est en Algérie que le Professeur A. Laveran devait découvrir, en 1880, l'agent de la fièvre paludéenne et établir ainsi sur une base réellement scientifique l'étude de cette affection en

[1] Général Duvivier, "Solution de la Question de l'Algérie." Paris, 1841, p. 4. Cette solution, c'était l'évacuation, p. 49.

the bush." The work of a mosquito brigade is making for the diminution of the gnats (*Stegomyia*) within the town and surrounding districts, whilst drainage of the lagoon has only just been decided upon.

(3). *German South-west Africa.*—This territory has badly suffered from disturbances, so that no precise statistics exist for this time. In 1907-1908 about 3,000 soldiers were sent home, and about 3,400 Europeans remained in March 1908. Among the latter occurred 507 cases of malaria with one death, and 4 cases of blackwater fever, one of which with lethal exitus. The malaria is more general in the north of the protectorate which projects into the tropics and possesses a pronounced rainy period, the rainfall measuring from 400 to 700 mm. per annum (December till May). During this short period the Anophelines multiply enormously in the numerous puddles and pools, even in the footprints of horses and wild animals and the leaf-axils of certain plants. As water is there a question of great economical importance, petrolisation is not practicable, as, for instance, in the case of watering-places for cattle. The only measure to be taken into consideration seems to be fumigation.

During 1906-1907 a proper quinine prophylaxis—every fifth and sixth, respectively eighth and ninth day a 0·5-1·0 gramme dose—was practised under the supervision of the local medical officers in the following stations: Namutoni, Zesfontain, Grootfontain, Waterberg, Outjo, Otavi and Omaruru. The result was a very unfavourable one: 11·6 to 90% of the persons treated suffered from malaria; in four stations more than 75%.

The reason for this non-success has not been quite explained, as since 1907 no further report has been published. In many places it has been stated that the quinine tablettes were not quite soluble; and it is probable that the form of malaria in question was of the most virulent type, against which the quinine was but of little use. The infection was mostly very slight.

It will be necessary to continue exact observations for many

years before one can possibly determine for what reason the quinine prophylaxis has failed just here. At the time any judgment upon this single observation would be premature.

(4). *The German possessions in the South Sea.*—The colonies in these regions are relatively small, and so is the number of Europeans. Only few medical officers are in practice there, and consequently the statistic material can add but little to what has been said above.

It is worth mentioning that *Koch* found only quartana on the "French Islands" (Bismarck-Archipel), and only tropica in Maty. Some groups of islands are completely free from malaria.

The results of anti-malarial measures in the German Colonies can be briefly stated as follows:—

1. The simplest and cheapest method has been found to be quininisation.
2. With carefully-practised and well-supervised quinine-therapy and prophylaxis it is possible to exterminate malaria even in very badly-infected places.
3. Individual prophylaxis with quinine suffices in susceptible persons (Europeans) to prevent with great certainty an outbreak of malaria. According to the probability of infection in a certain place, this prophylaxis must be increased or diminished.
4. Anti-malarial measures in the form of quinine treatment of infected persons have led to a decrease in the malaria morbidity, wherever they have been practised regularly, and for a sufficiently long period.
5. The steadier the native population of a place, the easier it will be to find out all infected people and treat them thoroughly.
6. Quinine must be distributed gratis, and directly offered to the natives.
7. The treatment of infected persons with quinine, according

to *Koch*, is sufficient to keep the malaria on a low level among the natives in tropical towns.

8. The treatment of the natives with quinine together with individual prophylaxis leads to a remarkable decrease in the malaria morbidity among Europeans.
9. In many cases quininisation alone will not suffice to reduce the morbidity to a degree which is desirable in the politico-economic interest.
10. Which one of these three anti-malarial measures, quininisation, mechanical protection, and anti-mosquito measures, is the most important can only be decided upon according to the local conditions.

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La culture intensive succédant à de grands travaux d'assainissement agricole, l'accroissement du bien-être et de l'instruction générale peuvent expliquer en partie ce recul du paludisme sur le territoire de la mère-patrie, mais il est difficile d'en préciser rétrospectivement d'une façon parfaite toutes les causes. La survivance des Anophélines au paludisme dans la France entière est un fait très net qui montre que le paludisme peut disparaître sans que son vecteur soit exterminé.

Malheureusement la France, grande puissance coloniale, a dans ses possessions d'au delà les mers un ennemi toujours présent dans le fléau paludéen.

I. *Campagnes antipaludiques en Algérie.*—La plus belle de ces colonies a vu son sort longtemps discuté, non seulement pour des raisons politiques diverses, mais surtout à cause de son insalubrité qui, croyait-on, devait en rendre à jamais le séjour impossible aux races européennes. Alger était occupé en 1830, et en 1841 le Général Duvivier, un héros de la conquête, écrivait la phrase fameuse : " Jusqu'à présent, les cimetières sont les seules colonies toujours croissantes, que l'Algérie présente."[1] C'est en Algérie que le Professeur A. Laveran devait découvrir, en 1880, l'agent de la fièvre paludéenne et établir ainsi sur une base réellement scientifique l'étude de cette affection en

[1] Général Duvivier, " Solution de la Question de l'Algérie." Paris, 1841, p. 4. Cette solution, c'était l'évacuation, p. 49.

même temps qu'il ouvrait un nouveau chapître de la microbiologie, celui des microbes pathogènes Protozoaires.[1] C'est aussi en Algérie qu'un autre médecin militaire : F. C. Maillot, dès 1834, apportait de l'ordre dans la détermination des fièvres continues des pays chauds, et, malgré de vives attaques de l'école broussaisienne, démontrait d'une façon définitive quelles sont les règles, jusqu'alors insoupçonnées, qui doivent présider à la médication quinique.[2] Deux chimistes français, Pelletier et Caventou, avaient rendu possibles les beaux travaux de Maillot en isolant l'alcaloide du quinquina. C'est à Maillot que l'Algérie doit d'être devenue française ; et le Dr Battarel a pu dire qu'il a complété de la façon la plus heureuse la formule du Maréchal Bugeaud : *ense et aratro* en y ajoutant : *et quina.*

L'Algérie était devenue habitable, mais le progrès normal de l'assainissement y est lent. Encore à présent, beaucoup trop de vies et d'énergies y sont consumées du fait du paludisme. C'est pourquoi, à l'annonce de la belle découverte de R. Ross, sur le rôle des moustiques dans l'étiologie paludéenne, l'Institut Pasteur de Paris envoya, en 1902, une mission antipaludique, dont les essais furent poursuivis d'abord sur les réseaux de Chemin de Fer. Mais dès 1904 M. le Gouverneur - Général Jonnart demanda à M. Roux, Directeur de l'Institut Pasteur de Paris, d'étendre ces essais aux localités les plus paludéennes de toute l'Algérie ; il leur témoigna une précieuse sollicitude, et les appuya constamment de sa haute autorité.

Territoire. — L'Algérie occupe une surface approximative de 300,000 kilomètres carrés. La partie habitable figure un parallélogramme allongé de l'est à l'ouest entre le Sahara et la Méditerranée. Sur le bord même de la mer surgissent des chaînes de montagnes dirigées également, d'une façon générale de l'est à l'ouest et dont les sommets dépassent rarement 2,000

[1] A. Laveran, "Traité du Paludisme." Paris, 2ème éd., 1907.

[2] Le Professeur Trolard à réuni un certain nombre des œuvres de Maillot dans un ouvrage de 260 pages. Paris, 1894, chez Doin.

mètres. Entre ces montagnes ou entre leurs contreforts s'ouvrent des vallées et des plaines (vallées de la Seybouse, de la Soummam, du Chéliff, plaines de la Mititdja, de la Macta, Hauts Plateaux), qui constituent autant de régions distinctes par leurs caractères de géographie physique. La faune, la flore, la climatologie sont ceux du bassin méditerranéen. Une assez étroite bande littorale limitée au Sud par la ligne qui correspond aux chutes d'eau minima annuelles de 40 cm. représente la partie colonisable par les Européens. Plus au Sud c'est le steppe, pays des Nomades, du mouton et du chameau. Plus au sud encore c'est le désert du Sahara, à climat continental, et qui sépare davantage l'Algérie de l'Afrique que la mer ne la sépare de l'Europe.

En raison de la faible hauteur des montagnes et de l'absence des neiges éternelles, le régime des cours d'eau (*oued* en arabe) est essentiellement torrentueux. Les marécages des plaines basses et les mares subsistant en été dans le lit des oueds sont les principaux gîtes à Anophélines.

Habitants.—Sur ce vaste espace habitent (recensement de 1906) 5,231,850 âmes, dont 729,960 Européens et 4,501,890 indigènes, les premiers agglomérés surtout dans les villes et les villages, les seconds épars dans la campagne. Les indigènes, tous de race blanche : Arabes ou Berbères arabisés dans les plaines, Berbères dans les montagnes ou le désert (Kabyles de la grande et de la petite Kabylie, Chaouias de l'Aurès, Touareg du Sahara), possèdent souvent ainsi que je l'ai montré avec mon frère, une certaine immunité relative contre l'hématozoaire du paludisme, qui leur permet d'héberger ce microbe dans leur sang périphérique, sans présenter de symptômes morbides (exactement comme les populations nègres de l'Afrique d'après ce qu' avaient déjà vu R. Koch, Stephens et Christophers). Cet état d'immunité relative ou de paludisme latent est très dangereux pour la collectivité car ces "porteurs d'hémotozoaires" qui souffrent peu, eux-mêmes, en état normal, de leur

infection, sont toutefois susceptibles d'infecter à tout moment les Anophélines.

Les sujets exposés au paludisme sont donc en premier lieu les colons immigrants, et les indigènes provenant de régions saines. Le degré de danger couru par eux dépend surtout du voisinage d'indigènes en état de paludisme latent, car il y a des Anophélines partout en Algérie, sauf dans les grandes villes. Le péril pour les colons établis dans une exploitation provient de ce que l'infection ne reste pas unique, mais est suivie, en règle, de réinfections successives qui se produisent avant que l'immunité relative ait pu être acquise. Les Européens qui résistent à ces réinfections présentent ensuite, comme les indigènes, une immunité relative coexistant avec un paludisme latent.

Anophélines.—Les Anophélines de l'Afrique du Nord actuellement connus sont :[1]—*Anopheles maculipennis* Meigen, l'espèce la plus répandue. On le trouve partout sauf dans les villes ; il habite le littoral, les plaines, les montagnes, les vallées, le steppe, le Sahara. En moyenne 4% sont infectés dans les pays paludéens.

A. algeriensis Theobald, espèce vicariante de *H. bifurcatus* espèce sylvane, farouche. On le trouve aussi infecté par des sporozoïtes de *Plasmodium.*

Pyretophorus myzomyifacies Theob. et Serg. Vallées des régions accidentées. On le trouve également infecté par des sporozoïtes de *Plasmodium.*

Pyretophorus sergenti Theob. Lisière des Steppes et du Sahara.

Pyretophorus chaudoyei Theob. (étudié par A. Billet), Sahara.

Une première épidémie de tierce bénigne apparaît au printemps, la tierce maligne prédomine en été et en automne. La quarte se montre surtout en automne et surtout dans certaines contrées.

[1] Voir *Annales Institut Pasteur*, t. xix., 1905, pp. 246 *sq.* ; t. xx., 1906, p. 248. *C. R. Soc. Biologie*, pp. 246, 1905, p. 499. F. V. Theobald, "Monograph of Mosquitos" (iv. tomes), London.

Mesures prophylactiques.—Nous les classons dans l'ordre suivant :—

A	*Eloignement* des gîtes à Anophélines	1
	Eloignement du Réservoir de virus	2
B	Grandes mesures *antilarvaires*	3
	Petites mesures antilarvaires	4
	Mesures contre les adultes	5
C	*Quininisation* préventive	6
	Quininisation curative	7
D	Défense *mécanique* personnelle	8
	Défense mécanique collective	9

Ces diverses mesures, partant de principes divers pour aboutir au même but, sont utilisables à des degrés différents selon que l'on se trouve dans l'un de ces trois cas :—

(1) Prophylaxie antipaludique d'un particulier.
(2) „ „ d'une collectivité.
(3) „ „ de l'État.

(1). *Prophylaxie d'un particulier.* — Dans les nombreux voyages que nous avons faits à travers les régions les plus paludéennes de l'Algérie, nous sommes restés toujours indemnes du paludisme en observant les précautions suivantes :—Autant que possible, passer la nuit loin des agglomérations indigènes et des gîtes à Anophélines. Prendre tous les jours une dragée de 0·20 centigrs. de bichlorhydrate de quinine (enrobée dans 0·30 centigrs. de sucre), pendant tout le temps que l'on passe en pays infecté, et pendant 15 jours après la rentrée en pays sain. Parfois, en pays très paludéen, nous prenions 0·40 centigrs. par jour, une dragée à chaque repas. Enfin nous n'avons jamais dormi en pays paludéen, soit la nuit, soit à l'heure de la sieste, sans notre moustiquaire portative que nous emportons toujours dans nos bagages et que nous montons toujours nous-mêmes.

Un certain nombre d'officiers, de fonctionnaires ou de colons contraints à traverser des régions fièvreuses ont adopté la pratique de ces mesures de préservation personnelle. Celle

qui est le plus fréquemment observée est la quininisation préventive.

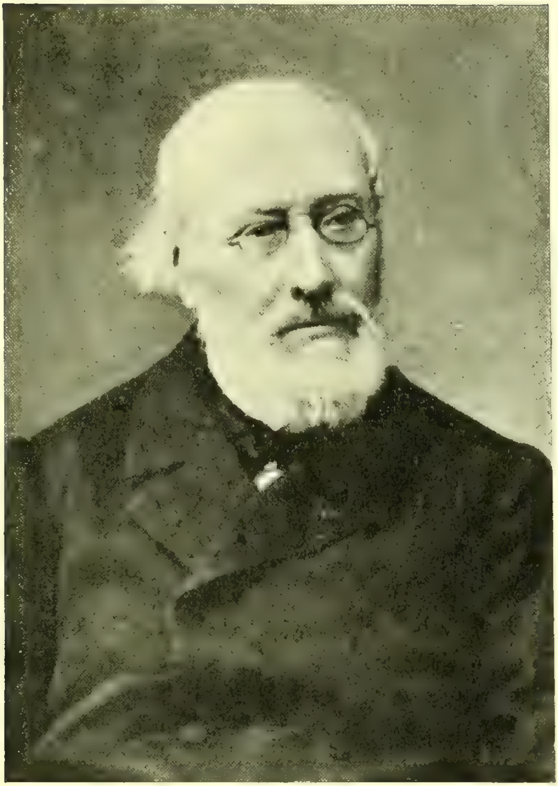
(2). *Une collectivité* (compagnie de chemins de fer, exploitations agricoles, minières, grandes administrations) doit éviter d'installer ses postes, ses gares, dans le voisinage des gîtes à Anophélines et des agglomérations indigènes. Le Service antipaludique en Algérie est consulté en principe chaque fois que l'on construit une ligne de chemin de fer et que l'on fonde un nouveau village de peuplement européen. On a pu faire prévaloir parfois le choix de certains emplacements moins exposés que d'autres à la contagion paludéenne.

Les collectivités (agents de chemins de fer et des Services publics, troupes, ouvriers agricoles, mineurs, etc.) peuvent être facilement soumises à une *quininisation préventive* par les petites doses journalières. Il est bon dans ces cas de faire tenir un registre des prises de quinine pour que l'on sache bien comment est absorbé le médicament.

Enfin ces collectivités peuvent être également protégées par les grillages métalliques posés aux ouvertures des habitations: *défense mécanique collective.*

C'est là un *procédé de luxe* qui coûte fort cher et ne peut être proposé qu'en faveur de personnes d'un niveau social assez élevé pour prendre soin de leur confortable. Nous tâchons de rendre cette défense mécanique le plus automatique possible, pour demander trés peu d'attention et de bonne volonté aux occupants des habitations. C'est ainsi que nous faisons établir les volets à l'intérieur des fenêtres de façon à ce que le grillage des fenêtres soit extérieur à tous les battants, et, par conséquent, n'ait pas besoin d'être percé de lucarnes pour le maniement des verrous, crochets au fléaux destinés à assujetir les volets. Si les volets sont extérieurs, par exemple dans les maisons déjà construites que l'on veut grillager, nous préférons pour les grillages les chassîs à guillotine dont la fermeture est automatique.

Fig. 1.



F. C. MAILLOT.

FIG. 2.


Pétrolage d'une mare,
gîte à Anophélines.
Drs Sergent reproduction autorisée

Fig. 3.

RECHERCHE ET PÉTROLAGE DES GÎTES À ANOPHÉLINES.

Fig. 4.

GÎTE À ANOPHÉLINE DANS UNE OASIS SAHARIENNE.

Fig. 5.

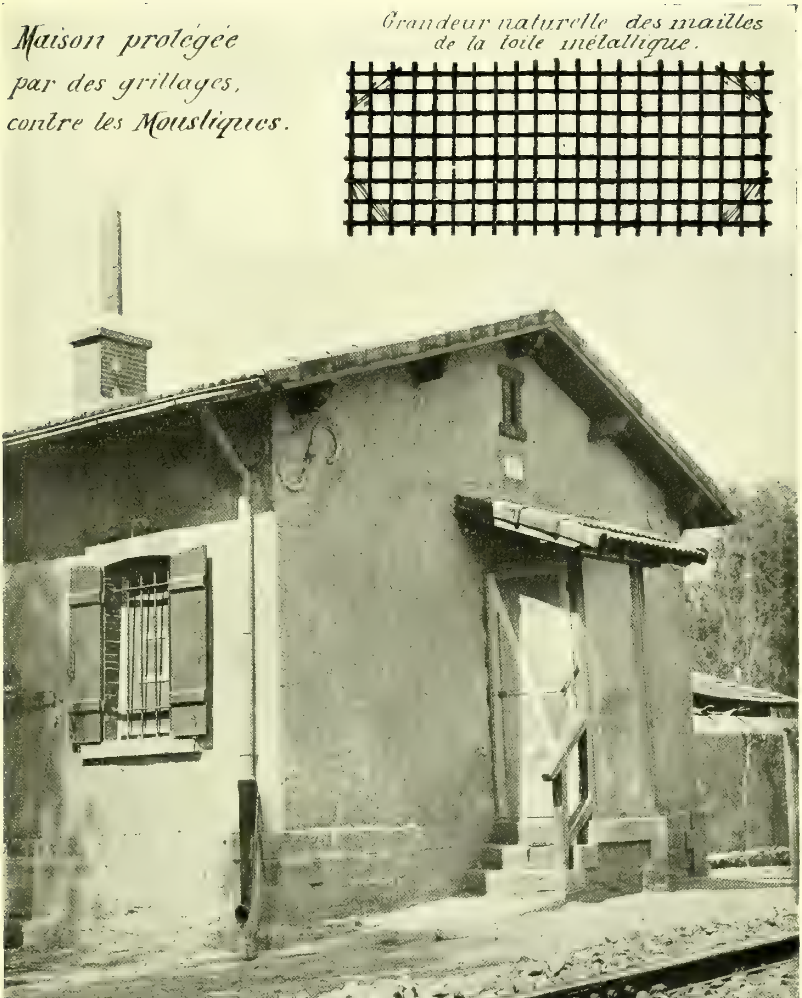
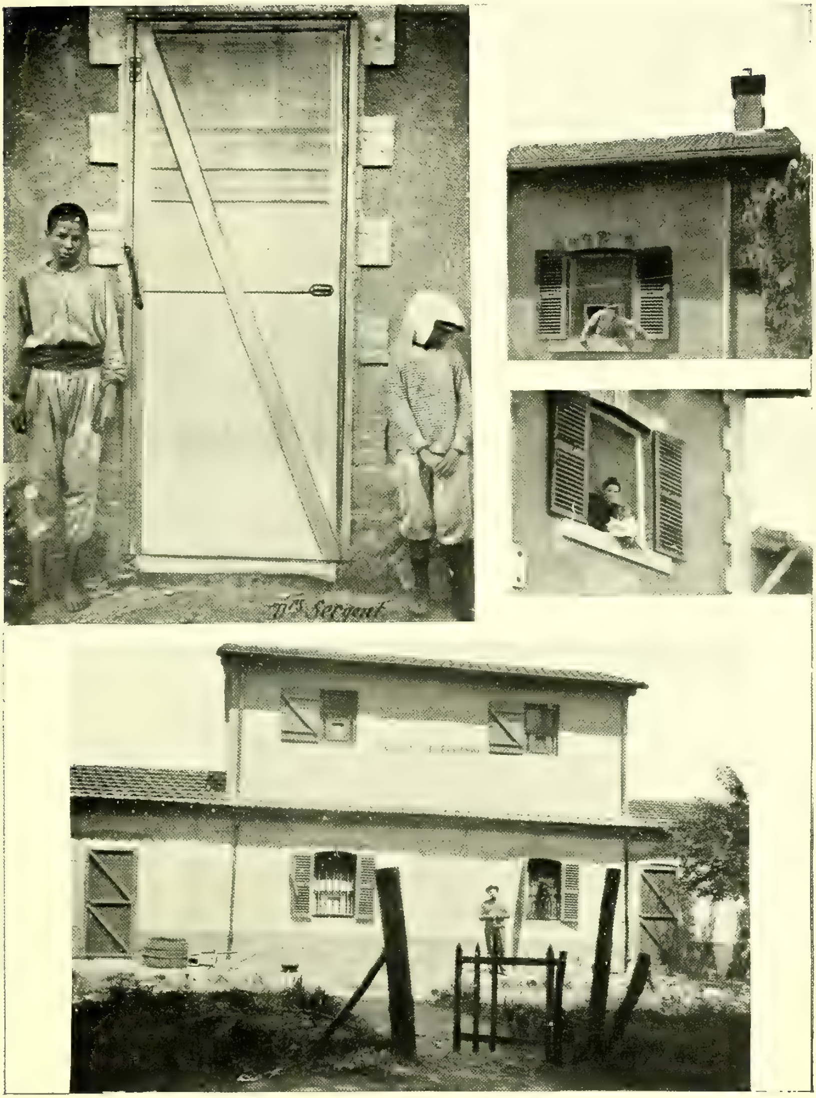

Maison protégée
par des grillages,
contre les Moustiques.
Grandeur naturelle des mailles
de la toile métallique.
Dr Sergent

FIG. 6.



DÉFENSE MÉCANIQUE (PORTES ET FENÊTRES GRILLAGÉES).

FIG. 7.

EXAMINATION OF CHILDREN.

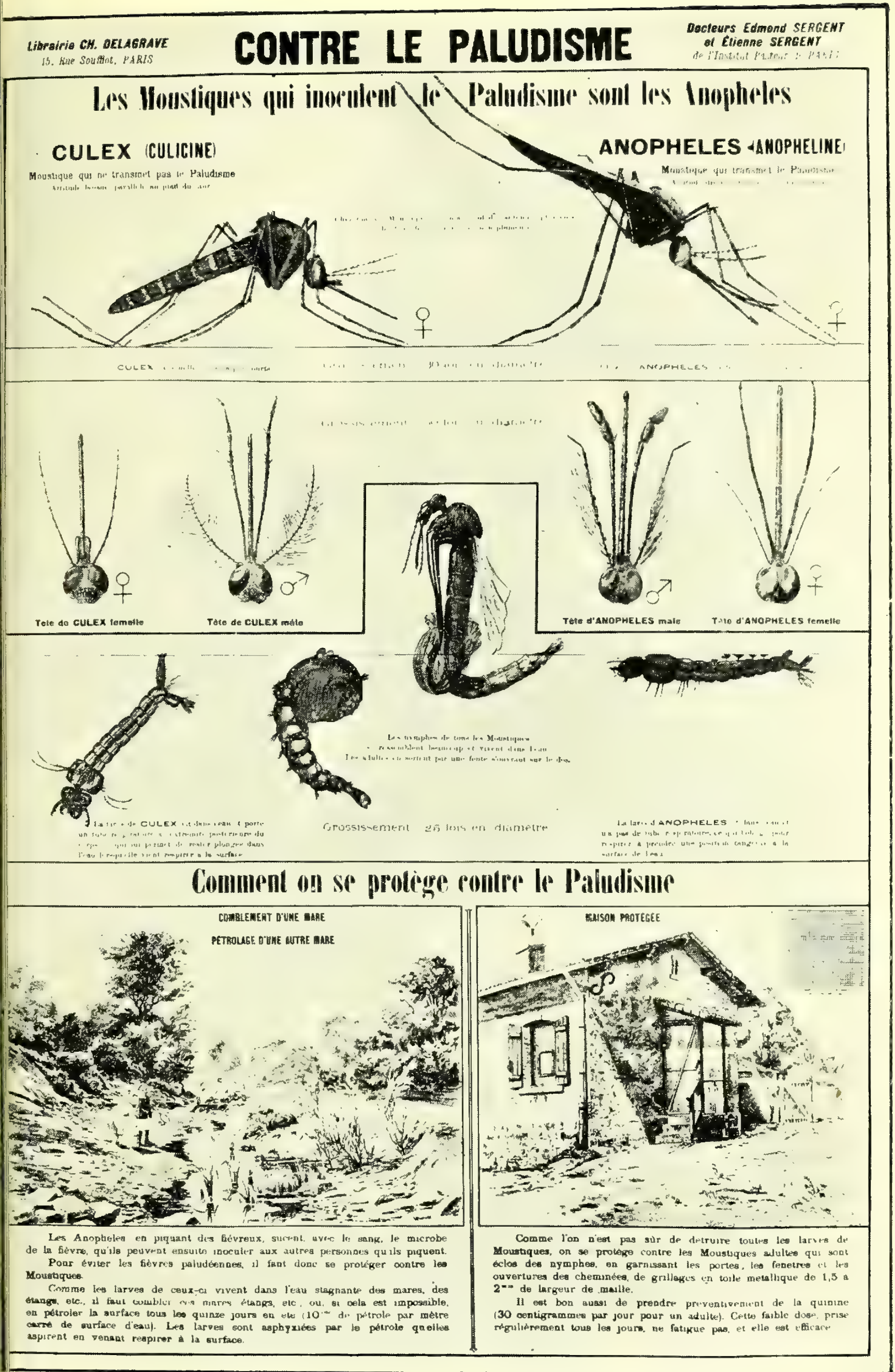


FIG. 8. SAMPLE CHART FOR PUBLIC INSTRUCTION.

[*To face page* 480.

(3). Le rôle de *l'État* dans la prophylaxie antipaludique peut être compris ainsi:

Exécution des *grandes mesures antilarvaires*:—Canaux de déssèchement, grandes plantations dans les marais, reboisement des montagnes, petits barrages dans les hauts bassins, régularisation des cours d'eau, surveillance des barrages, réservoirs, etc. Mais ces énormes travaux, qui coûtent très cher, ont toujours un but principal d'assainissement agricole; l'hygiène antipaludique ne peut intervenir ici qu'à titre accessoire, pour appuyer tel projet, contre-indiquer tel autre. D'ailleurs ces grandes mesures antilarvaires ne sont pas complètement efficaces par elles-mêmes; elles ne font que rendre possibles les petites mesures antilarvaires et surtout la bonification agricole. Celle-ci, en draînant et en perméabilisant le sol, en supprimant les moindres mares, en utilisant toute l'eau des pluies et toute l'eau des sources, aboutit, en poursuivant un but purement cultural, à un résultat réellement antipaludique. Nous avons souvent constaté qu'un grand canal de déssèchement est surpeuplé de larves d'Anophélines, tout comme le marais qu'il remplace. Mais le marais était inaccessible, tandis que des deux berges du canal on peut atteindre et détruire les larves d'Anophélines.

Cet exemple montre bien ce que sont, à notre sens, les petites mesures antilarvaires. Dans le canal dont il s'agit, les larves réapparaîtront constamment, quoiqu'on fasse, car il aura toujours de l'eau, puisque c'est son office de drainer l'eau des couches superficielles du sol. D'autre part il viendra toujours des Anophélines femelles pondre sur cette eau tranquille, car si loin que l'on recule les limites des mesures antilarvaires, elles ne pourront pas englober toute l'Afrique du Nord. Par conséquent les mesures de destruction des larves devront être répétées indéfiniment sur les parties du canal voisines de lieux habités (dans un rayon de un kilomètre et demi) et c'est là le principal caractère des petites mesures antilarvaires: de n'avoir qu'une efficacité temporaire. Elles

tiennent purement et simplement en échec la pullulation des larves pendant le temps où elles sont mises à exécution. On a donc intérêt à maintenir indemnes de larves les environs d'une agglomération importante, mais il faut évidemment que la valeur économique du groupement à défendre soit assez grande pour justifier les dépenses engagées. Il est une autre utilité des petites mesures antilarvaires, c'est celle de supprimer temporairement les vecteurs de la contagion et, par suite, d'empêcher pendant ce temps les réinfections. Elles permettent ainsi la guérison des anciens infectés, que l'on aide, d'autre part, par la quininisation curative. En résumé, à mon sens, *les petites mesures antilarvaires auront pour but, non pas seulement d'éviter les infections des indemnes, mais aussi de permettre la guérison des anciens infectés.*

Le type des petites mesures antilarvaires est présenté par les faucardements des eaux stagnantes répétés, suivant les besoins, 2 ou 3 fois par an, et les pétrolages, renouvelés tous les quinze jours (tous les 8 jours au Sahara). Mais les minimes artifices, qui rendent impossible la vie des larves dans les aquariums parfois très réduits qui leur suffisent, peuvent être variés à l'infini. Je citerai comme un exemple typique le procédé suivant que nous avons appliqué déjà souvent.

Soit une source, une fontaine, un abreuvoir, dont l'eau d'écoulement forme des gîtes intarrissables et permanents. Nous faisons creuser, au lieu d'un seul canal, deux petits canaux d'écoulement fermés à leur origine par une vanne. On laisse l'eau s'écouler par un seul canal pendant huit jours: les œufs y sont pondus, les larves naissent, se développent. Au bout de huit jours nous baissons la vanne de ce canal et ouvrons l'autre : les larves qui ne sont même pas encore arrivées à la nymphose meurent sur la boue qui sèche au fond du canal abandonné par l'eau. Ainsi, sans aucune surveillance, cinq minutes de travail toutes les semaines suffisent à empêcher tout développement des larves dans cette eau d'écoulement.

Les mesures contre les adultes telles que les fumigations de

poudre de pyrèthre ou de soufre, la pulvérisation de poudre de pyréthre ou de pétrole, les trous-pièges de Blin, jouent un rôle très secondaire dans la lutte antipaludique.

C'est au contraire la *quininisation curative* que l'État peut appliquer en grand et faire servir le plus efficacement à l'assainissement d'une région en Algérie. Nous dirons d'abord que dans notre pratique nous organisons la quininisation curative exactement comme la quininisation préventive. Il faut, en effet, réserver aux médecins le traitement des fièvreux, vraiment médical, c'est à dire celui qui procède par des fortes doses. D'autre part, il est bien plus commode d'avoir une technique unique qui s'applique aussi bien aux sujets indemnes qu'aux anciens infectés dans un pays où le paludisme est aussi commun. Enfin la comparaison des index endémiques par la palpation des rates avant et après les campagnes antipaludiques nous démontre la réelle efficacité des petites doses journalières longtemps répétées. D'ailleurs on sait que même les petites doses de quinine suffisent à faire disparaître les parasites du sang périphérique bien avant la guérison totale de l'infection paludéenne. Les individus ainsi "blanchis," alors même qu'ils ne sont pas encore guéris, sont déjà rendus inoffensifs pour la collectivité Au moment où les procédés de la quininisation préventive et curative sont mis en œuvre de plus en plus dans tous les pays paludéens, on nous permettra de rappeler que depuis longtemps en Algérie, à la suite de Maillot, les médecins civils et les médecins de l'armée ont préconisé et appliqué ces deux sortes de quininisation. Je citerai, parmi les expériences les plus nettes, celles de Sezary[1] et celles de Cornebois.[2] Sezary écrit en 1891 : "J'ai fait prendre du Ier Juillet au Ier Décembre, tous les jours, à l'un des repas 15 centigrammes de quinine, quelquefois 20, mais jamais davantage. Avec cette dose infime, j'ai maintenu, depuis deux ans, en divers centres,

[1] "Prophylaxie de la Malaria par la Quinine." Alger, Jourdan, 16 pp., 21 Octobre 1891.

[2] "Contribution à l'Étude de la Prophylaxie de la Malaria par la Quinine." Thèse médecine, Montpellier, 1894.

très malsains, des familles entières en parfait état de santé, au milieu de voisins toujours malades. Et non seulement les accès disparaissent, mais l'appétit augmente, les couleurs du visage, l'embonpoint reviennent, en un mot, c'est l'apparence de la santé parfaite . . . tantis qu'autour de ces personnes tout le monde subit les influences fâcheuses du milieu."

Cornebois arrive en 1894 exactement aux mêmes conclusions : " L'Administration de petites doses quotidiennes de quinine a pour résultat de prévenir, soit l'invasion, soit le retour du paludisme."

Les médecins de l'armée emploient depuis longtemps en Algérie la quininisation régulière préventive, et les résultats dépendent du soin qu'on apporte à observer leurs prescriptions.

Le Service antipaludique en Algérie emploie des dragées lenticulaires roses de bichlorhydrate de quinine, semblables à celles de l'État italien et contenant vingt centigrammes de sel comprimés et enrobés dans trente centigrammes de sucre. Nous faisons distribuer une dragée par jour aux adultes (deux dragées dans les régions très paludéennes) ou bien trois dragées tous les deux jours, ou tous les trois jours.

Pour assurer une exacte administration de la quinine, éviter tout abus et tout accident, nous croyons nécessaire de faire distribuer les dragées par des quininisateurs, agents des services publics ou personnes de confiance, chargées temporairement de ces fonctions pendant l'été, moyennant rétribution. Les quininisatrices sont encore plus précieuses que les quininisateurs, à cause de leur facile accès auprès des femmes musulmanes. Le quininisateur distribue lui-même aux moments fixés : tous les jours, ou tous les 2 jours, ou tous les 3 jours, la quinine aux personnes inscrites sur un registre *ad hoc*. Il fait ingérer les dragées en sa présence et note les prises sur son registre.

Nous évaluons les résultats de cette quininisation par la comparaison des index endémiques avant et après la campagne. En règle générale ce sont les *index spléniques* que nous relevons, et, nous ne quininisons, d'habitude, que les agglomérations où

l'index splénique dépasse, au printemps, 15%. Lorsqu'on le peut, on relève aussi *l'index hématologique*.

La quininisation en grand par l'État est rendue difficile en Algérie par le fait que ce pays est soumis comme la France européenne à la loi de Germinal an XI qui donne aux Pharmaciens le monopole de la vente de la quinine. Il a fallu donc que le Gouvernement Général de l'Algérie négocie avec les Pharmaciens de la Colonie une entente aux termes de laquelle les dragées de bichlorhydrate de quinine vendues au public, sous des marques de garantie spéciales, sont soumies à un tarif particulier, aussi bas que possible quoique laissant un bénéfice aux Pharmaciens. Ceux-ci approvisionnent de ces dragées des dépôts de quinine confiés à des agents des Services publics autorisés à revendre, au prix marqué, les dragées au public. D'autre part les Pharmaciens délivrent ces dragées au Service antipaludique, pour la quininisation du Réservoir de virus, sans prélever de bénéfice. En somme cette entente, si elle donne ses fruits, mettra à la disposition du public, dans toute l'Algérie, de la bonne quinine, à bon marché, sous une forme agréable, et permettra à l'État d'entreprendre avec le moins de frais possible la quininisation publique de l'énorme Réservoir de virus constitué par des millions d'indigènes, en majorité indigents.

Si nous voulons résumer le mode d'action de l'État Algérien dans la lutte antipaludique nous dirons que ses efforts principaux portent sur la réduction du Réservoir de virus formé par les indigènes anciens infectés. Cette réduction du Réservoir de virus est poursuivie par la quininisation par petites doses quotidiennes, et elle est facilitée, accélérée dans toutes les localités où la chose est possible, par l'organisation de petites mesures antilarvaires.

Le service antipaludique a institué des campagnes antipaludiques expérimentales dans trois champs de démonstration : un par département : Montebello (Alger), Tourville (Oran), Mondovi (Constantine). D'autre part il subventionne et aide

les campagnes locales organisées suivant les mêmes principes par les médecins collaborateurs résidant dans des villages fiévreux et dirige les mesures prises sur les réseaux des chemins de fer, dans les immeubles administratifs. Enfin il propage les connaissances nouvelles sur l'étiologie et la prophylaxie du paludisme.

(Une ligue algérienne contre le paludisme, fondée en 1903 par les Prof. Moreau et Soulié se livre également à la vulgarisation des découvertes récentes sur l'étiologie et la prophylaxie du paludisme.)

Résultats.—Nous résumons ci-dessous les résultats constatés dans les champs de démonstration du Service antipaludique. Ils donnent une idée des résultats obtenus par les diverses campagnes menées ailleurs.

(1) *Département d'Alger.—Montebello* (mesures antilarvaires et quininisation).

Avant la campagne :—

En 1904 75 infectés, 12 indemnes, il se produit 11 Ières infections.

Après la campagne :—

En 1905	44	anciens	infectés,	36	indemnes,	il se	produit	0	Ière	infection.
„ 1906	21	„	„	71	„	„	„	0	„	„
„ 1907	13	„	„	65	„	„	„	0	„	„
„ 1908	20	„	„	54	„	„	„	0	„	„
„ 1909	18	„	„	68	„	„	„	0	„	„

Depuis le début de la campagne (1905) il y a eu à Montebello 10 naissances. Aucun de ces enfants n'a eu le paludisme jusqu'à présent. Le fait est unique dans l'histoire de Montebello.

Localités du voisinage, non défendues, pouvant servir de témoins :—

En 1905 9 Personnes indemnes, 8 premières infections.
„ 1906 29 cas graves de première invasion (1 mort par accès pernicieux).
„ 1907 7 personnes indemnes, 4 premières infections.
„ 1908 3 personnes indemnes, 1 cas de première invasion.
„ 1909 5 personnes indemnes, 2 cas de première invasion.

(2) *Département d'Oran.* — *Tourville* (mesures antilarvaires et quininisation).

Avant la campagne :—

En 1905 Sur 82 enfants, 58 grosses rates (70·7%).

Après la campagne :—

En 1906 Sur 113 enfants, 26 grosses rates (23%). Sur 900 habitants 1 Ire infection.
„ 1907 Sur 65 enfants, 4 grosses rates (6·1%). Sur 1000 habitants 1 Ire infection.
„ 1908 Sur 148 enfants, 5 grosses rates (3·3%). Sur 1000 habitants 0 Ire infection.
„ 1909 Sur 188 enfants, 8 grosses rates (4·2%). Sur 973 habitants 0 Ire infection.

Depuis le début de la campagne (1906), il y a eu à Tourville 106 naissances. Deux enfants seulement se contaminèrent (1 en 1906, 1 en 1907).

Sainte Léonie. — (Mesures antilarvaires et quininisation—celle-ci défectueuse).

Avant la campagne :—

En 1905 Sur 30 enfants, 15 grosses rates.
„ 1906 Sur 49 enfants, 18 grosses rates (36%). Sur 300 habitants 2 Ires infection dont une mortelle.

Après la campagne :—

En 1907 Sur 60 enfants, 16 grosses rates (25%). Sur 300 habitants 0 Ire infection.
„ 1908 Sur 78 enfants, 10 grosses rates (12%). Sur 300 habitants 2 Ire infection.
„ 1909 Sur 127 enfants, 7 grosses rates (4·1%). Sur 346 habitants 0 Ire infection.

Localités du voisinage, non défendues, pouvant servir de témoins :—

En 1906 14 personnes indemnes, 14 premières infections.
„ 1907 2 „ „ 2 „ „ dont une mortelle.
„ 1908 1 personne indemne, 1 première infection.
„ 1909 1 „ „ 1 „ „

(3) *Département de Constantine.* — *Mondovi* (mesures antilarvaires et quininisation).

Avant la campagne :—

En 1906 Plusieurs cas d'hémoglobinurie et plusieurs cas de première invasion chez les nouveau-nés.

Après la campagne :—

En 1907	Sur	773	personnes	3	cas	de première	invasion.
„ 1908	„	1300	„	2	„	„	„
„ 1909	„	1300	„	2	„	„	

L'index endémique de Mondovi :—

En	automne	1906	est	de	78·8%
„	„	1907	„	„	42·5%
„	„	1908	„	„	32·6%
„	„	1909	„	„	28·5%

Localités voisines, pouvant servir de témoins :—

En 1907	19 cas de première invasion.
„ 1908	Nombreuses rechutes graves.
„ 1909	Sur 260 personnes, 2 cas de première invasion.

Penthièvre.—(Mesures antilarvaires et quininisation).

Après la campagne :—

En 1907	Sur	200	Européens	2 cas de première invasion (bénins).
„ 1908	„	200	„	1 cas bénin de première invasion.
„ 1909	„	200	„	0 cas de première invasion.

L'index endémique de Penthièvre :—

En	automne	1907	est	de	40 %
„	„	1908	„	„	32·1%
„	„	1909	„	„	23·5%

Localités voisines, témoins de Penthièvre :—

En 1907	1 cas de première invasion.
„ 1908	Cas de paludisme nombreux.
„ 1909	Sur 260 personnes, 2 cas de première invasion

En 1909 ont été distribués, en dehors des champs de démonstration, 450 kilos de dragées de quinine, par les médecins collaborateurs du Service antipaludique, qui avec un zèle et un désintéressement dont on ne saurait trop leur savoir gré, ont ajouté à leurs occupations professionnelles la charge de com-

battre l'épidémie la plus répandue, sinon la plus grave actuellement, en Algérie.

Budget de la lutte antipaludique en Algérie.—Depuis plusieurs années, les campagnes antipaludiques coûtent au Gouvernement Général environ cinquante mille francs par an: Personnel permanent (avec frais de déplacement) environ 16,000 francs. Personnel temporaire (quininisateurs): de 3,000 à 4,000 francs. Achat de quinine: de 18 à 20,000 francs. Petites mesures antilarvaires: de 13 à 15,000 francs. La défense mécanique est payée par chaque administration compétente.

Le budget total de l'Algérie est de cent millions de francs environ.

II. *Campagnes antipaludiques en Tunisie.*—La Tunisie, qui continue, à l'est, l'Algérie, nourrit sur 100,000 kilomètres carrés 1,800,000 habitants dont 1,600,000 indigènes.

Comme en Algérie, presque toutes les régions basses et arrosées, les plaines marécageuses, les vallées parsemées de sources sont frappées par le paludisme au printemps (épidémie de tierce benigne) et en été et automne (fièvre maligne estivo-automnale). Certaines régions présentent de la quarte.

Les Anophélines signalés sont: *Anopheles maculipennis* Meigen, *Anopheles algeriensis* Theobald, *Pyretophorus chaudoyei* Theobald.

C. Nicolle, après une première expérience de lutte antipaludique en 1903, obtint la création d'un service antipaludique rattaché à l'Institut Pasteur de Tunis, et qui fonctionne régulièrement depuis Janvier 1907, sous la direction technique de Husson.[1] Ce service a pour but principal de défendre contre le paludisme les centres de colonisation en enseignant aux colons les dangers qu'ils courent et les précautions à prendre pour les éviter et en renseignant l'administration sur les mesures prophylactiques d'ordre général ou local qu'elle seule peut entreprendre. En conséquence, des tournées sont faites dans un but

[1] Voir *Archives de l'Institut Pasteur de Tunis*, depuis 1906, et *Bulletin de la Soc. de Pathol. Exotiq.*, t. i., 1908, p. 385 *sq.*

de propagande et d'organisation dans les localités les plus malsaines: il faut citer en particulier les défenses organisées sur les réseaux des chemins de fer et dans des compagnies d'exploitation des phosphates, les règlements élaborés pour la déclaration obligatoire du paludisme dans la ville de Tunis. Pour un certain nombre de localités les conditions exactes du paludisme ont été déterminées. Pour propager les principes de la lutte antipaludique, des distributions sont faites de brochures, d'affiches. Enfin le service antipaludique ainsi que le gouvernement tunisien distribue gratuitement des dragées de quinine.

Le privilège des pharmaciens n'existant pas en Tunisie, pays de protectorat, l'État a pu y organiser la vente de la quinine. Un arrêté du 15 Juin 1909 charge les débitants de tabac de la vente de la quinine dont ils s'approvisionnent à l'entrepôt des Monopoles. Le minimum d'approvisionnement et le prix de vente sont réglementés. Il s'agit du chlorhydrate de quinine en dragées de 20 centigrammes.

Sur les lignes de chemins de fer, où les mesures sont bien prises et surveillées, on constate l'assainissement réel de gares et maisonnettes auparavant fort paludéennes. Dans les centres de colonisation, où l'intensité du paludisme est plus difficile à mesurer exactement, on constate les progrès de l'éducation du public, et une amélioration souvent très nette de l'état paludéen.

Budget. — Le Budget officiel de l'antipaludisme monte à quelques milliers de francs (appointements et frais de tournée du Directeur du service, achat de quinine, publications de propagande).

III. *Campagnes antipaludiques en Corse.*[1] — *Territoire.* — La Corse est une île allongée du Nord au Sud sur 200 kilomètres de longueur et une largeur maxima de 90 kilomètres. Une chaîne de montagnes dirigée selon le grand axe Nord-Sud est séparée

[1] Les renseignements qui suivent m'ont été très aimablement communiquées par le Dr Thiers, président de la Ligue Corse contre le Paludisme, que je remercie très vivement de sa grande obligeance.

de la mer à l'est par une plaine longue de 150 kilomètres et large de 6 à 20 kilomètres. Cette plaine orientale couvre 190,000 hectares, près du quart de la superficie de l'île. Elle est toute entière infectée de paludisme, à type estivo-automnal.

Population.—Le Dr Thiers estime à 40,000 le nombre des habitants qui subissent annuellement les atteintes du paludisme (sur un chiffre total de population qui n'atteint pas 300,000 âmes) pour le département tout entier.

Anophélines.—L'espèce dominante est *Anopheles maculipennis* Meigen.

Mesures adoptées.—L'État n'a encore pris aucune mesure importante en Corse. Ce fut le Professeur A. Laveran qui, en 1901,[1] prit l'initiative de conseiller la création d'une société locale pour l'assainissement de ce pays. Une ligue corse contre le paludisme fut fondée en 1902 par le Dr Battesti, et depuis la mort de ce regretté savant, elle est présidée par le Dr Thiers.

L'action des dévoués Sociétaires est surtout morale, elle consiste d'abord en une œuvre de diffusion dans le département de brochures de vulgarisation, envoi de cartes postales, pose d'affiches mettant en garde contre la piqûre des moustiques, conseils sur la façon de prendre la quinine, renseignements sur la façon d'organiser la production mécanique, installation et entretien de quelques maisons protégées, destinées à servir de modèle.[2]

La Ligue a obtenu des pharmaciens que ses adhérents puissent se procurer la quinine au prix de quinze centimes le gramme, prix bien inférieur aux prix en cours auparavant. Elle a décidé la Compagnie des Chemins de Fer départementaux d'abord, les diverses Administrations de l'État ensuite, à

[1] *Bull. Ac. Médecine*: "Sur la Prophylaxie du Paludisme en Corse," 24 Décembre 1901; et "Assainissement de la Corse," 7 Octobre 1902. Voir Battesti: "Ligue Corse contre le Paludisme." *Campagne* 1902. Bastia 1903. "La 2ème Campagne de la Ligue Corse contre le Paludisme." Année 1903. Bastia 1903.

[2] Voir la communication des Drs Thiers e Stefani au *Congrès de l'Alliance d'Hygiène Sociale*. Lyon, Mai 1907.

adopter les toiles métalliques et la prophylaxie quinique dans les localités réputées paludéennes.

Résultats obtenus.—Au point de vue moral, l'opinion publique en Corse est avertie, à l'heure actuelle, des dangers et des causes du paludisme, ainsi que des moyens d'y remédier. Au point de vue pratique la morbidité a diminué en certains points bien surveillés : elle est tombée parfois de 50% à 12%, mais il ne s'agit que des cas de première invasion, les récidives sont encore aussi nombreuses. C'est que le zèle des médecins qui dirigent la lutte antipaludique et des Chefs d'Administration n'est pas toujours secondé par la bonne volonté des intéressés eux-mêmes. La question du paludisme devrait constituer une des principales préoccupations de la Commission gouvernementale chargée actuellement d'étudier les moyens du relèvement économique de la Corse.

Budget. — Les cotisations des Sociétaires ont subventionné l'œuvre de la Ligue Corse ainsi qu'une somme de 3,000 francs que lui attribua le Professeur Laveran sur des fonds mis à sa disposition par la caisse des recherches scientifiques.

IV. *Campagnes Antipaludiques dans les Colonies Françaises.*—Le Ministère des Colonies a publié en 1903 des *Instructions concernant les mesures à prendre contre les maladies endémiques, épidémiques et contagieuses.* Mr le Médecin Inspecteur Général Kermorgant y consacre 22 pages à la hygiène antipaludique.[1] Il revient en 1906 sur cette question dans un article sur "La Prophylaxie du Paludisme."[2] Sur ses instructions, l'étude des Culicides fut poursuivie dans toutes les colonies Françaises et montra, entre autres choses, que les seules colonies exemptes de paludisme: la Nouvelle-Calédonie et Tahiti, sont en même temps indemnes d'Anophélines.[3]

Mais on constate surtout des essais *individuels* de grillages des habitations. Et d'autre part, depuis longtemps, l'initiative

[1] Voir *Annexe aux Annales d'Hygiène et de Médecine Coloniale*, 1903.

[2] *Ann. d'Hyg. et Médec. Colon.*, t. ix., 1906, pp. 18-46.

[3] Je remercie vivement Mr le Médecin Inspecteur Général Kermorgant des renseignements qu'il a bien voulu me communiquer.

de prescrire de la quinine préventive dans les pays paludéens ou en expéditions coloniales est laissée aux médecins.[1]

Les renseignements que nous avons pu recueillir parmi les documents publiés et au Ministère des Colonies sont les suivants:—

La Réunion.—Ile de 2,500 kilomètres carrés et 170,000 habitants infectée depuis 1867 par le paludisme. Deux espèces d'Anophélines : *Myzorhynchus coustani* et *Pyretophorus costalis.*

En 1902 le Colonel Sordoillet, commandant supérieur des troupes édicta des règlements, qui furent bien appliqués dans la population militaire, pour la quininisation préventive et curative, l'emploi obligatoire des moustiquaires et la destruction des moustiques dans le voisinage des postes et casernes.

La mortalité autrefois assez élevée dans les statistiques militaires devient nulle en 1904 et 1905, alors que la mortalité dans la population civile subissait une progression importante.

Après le départ de cet officier supérieur, les mesures ne furent pas aussi bien surveillées et les atteintes de paludisme recommencèrent.[2]

Madagascar.—Un décret du 20 Octobre 1905 autorise le Gouverneur-Général de Madagascar à prendre par voie d'arrêté toutes les dispositions nécessaires pour que les sels de quinine soient mis à la disposition de tous et au plus bas prix possible.

Le Professeur A. Laveran fait émettre en 1904 un voeu par l'Académie de Médecine pour que les casernements militaires de Madagascar soient protégés par des toiles métalliques.[3]

Afrique occidentale française.—Sénégal.—Le Dr Thiroux qui, après le Dr Marchoux a spécialement étudié le paludisme du Sénégal, rapporte[4] les mesures prises pour assainir en 1904-1905 la ville de Saint-Louis (20,000 habitants).

[1] Voir A. Laveran, "Traité du Paludisme." 2e éd., 1907.

A. Billet, "La Lutte contre la Malaria en France et dans les Possessions Françaises en 1903." *Atti. de Soc. per gli Studi della Malaria*, t. v., 1904, p. 287.

[2] J. J. Vassal, "Le Paludisme à l'Ile de la Réunion." *Atti. de Soc. per gli Studi della Malaria*, t. viii., 1907, p. 19.

[3] *Bull. Acad. Médec.*, "Séance du 8 Mars 1904," pp. 183-190.

[4] A. Thiroux et L. d'Anfreville, "Le Paludisme au Sénégal pendant les Années 1905-1906."

J. B. Baillière, 1908. 57pp. Pl. en couleurs

Les Anophélines du Sénégal sont :—*Pyretophorus costalis*, *Myzomyia funesta*, *Myzorhynchus paludis*, *Cellia pharoensis*, *Cellia squamosa.*

On procéda au remblaiement des marais de la pointe nord de la ville de Saint-Louis, des marais du faubourg de Sor. Pour ces derniers, on a installé un système de drainage, avec siphons s'ammorçant d'eux-mêmes à marée basse et ne fonctionnant plus à marée haute. On procède à des pétrolages dans la ville de Saint-Louis ; quelques essais de défense mécanique ont été tentés. La ligne du chemin de fer de Saint-Louis à Dakar a toutes ses gares et ses logements d'employés protégés par des toiles métalliques.

Territoires des Rivières du Sud.—Le Dr Le Moal appliqua en 1905 [1] les mesures antilarvaires à l'assainissement de Conakry (900 habitants) bâtie sur l'île de Tumbo, une des villes les plus paludéennes d'Afrique. Il y organisa des brigades à moustiques qui eurent pour mission de remblayer les bas-fonds, de drainer les marais, de pétroler les mares.

Haut-Sénégal et Niger. — Le Dr Bouffard [2] conseille, avec exemples à l'appui, pour l'Européen la cure quinique préventive (dose quotidienne de 25 centigrs. augmentée en cas de danger spécial) prolongée deux semaine après le retour en pays sain.

Il y joint l'usage de la moustiquaire individuelle.

Il estime par contre la quininisation des indigènes impossible dans ces pays et croit les mesures antilarvaires susceptibles de diminuer le paludisme chez les Nègres. Il trace un programme des travaux à exécuter suivant que les agglomérations indigènes sont en plaine ou en montagne. Enfin l'Adminis-

[1] Le Moal, "Campagne Antipaludique à Conakry en 1905." *Ann. Hyg. et Méd. Colon.*, t. ix., 1906, pp. 550.

Voir aussi du même auteur : " Étude sur les Moustiques en Afrique Occidentale Française, Rôle Pathogène-Prophylaxie." *Ibidem*, 1906, pp. 181-219.

[2] Bouffard, " Prophylaxie du Paludisme chez l'Européen dans le Haut-Sénégal et Niger." *Bull. Soc. Path. Exot.*, t. ii., 1909, No. 1, pp. 34-39.

" Prophylaxie du Paludisme chez l'Indigène dans le Haut-Sénégal et Niger." *Ibidem*, t. ii., 1909, No. 2, pp. 84-87.

tration peut agir efficacement en choisissant d'après les règles de l'hygiène antipaludique l'emplacement des villages.

Indochine.—Comme pour Madagascar, un décret autorise le Gouverneur-Général à délivrer aux indigènes de la quinine gratuitement ou à très bon marché.

By H. C. ROSS, M.R.C.S., L.R.C.P.

Director of the Royal Southern Hospital Researches, Liverpool
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53. Mosquito Reduction in Egypt.—Before considering the history and details of the several campaigns started and in progress in Egypt, it will be better to discuss briefly the local conditions which affect a sanitary measure of this nature. Egypt owes its very existence to the Nile. Were it not for that river Egyptian territory would merely be a part of the desert. Except on the Mediterranean Coast in the winter months, rainfall is a negligible quantity; so that stagnant pools of rain-water like those found in the tropics are unknown. The only natural collections of water in the country are the Nile, the two Bitter Lakes through which the Suez Canal passes, a fresh-water lake — derived from the Nile—at the Oasis of the Fayoum, and two shallow sea-water lakes near the Mediterranean Coast. The Nile itself flows through Egypt without receiving a single tributary.

Although the natural collections of water are so few, Egypt may be defined as a veritable marsh surrounded by desert. The marsh is, of course, confined to the narrow limits of the basin of the Nile, and the water is the most valuable asset of the country. Canals and irrigation channels are everywhere, and the native spends his existence baling the water either out of the Nile itself, or from the canals, or from wells, on to the soil itself. In consequence, if there are few natural collections of water, the country is riddled with puddles and artificial breeding-places for mosquitos.

The peculiar cosmopolitan nature of the population must

be mentioned. The term "native" refers to the local subject, and it is used in contradistinction to the Europeans, many of whom, so far as their instincts regarding sanitation are concerned, are not very superior to the native. Almost every nationality finds a home in Egypt, and foreign countries do not appear always to send their best subjects when Egypt is singled out as a settlement for them. The native himself is fairly easily led, especially the lower classes; but when he becomes educated, especially in Europe, he is inclined to form ideas for himself which are not always of the best; and it is then frequently necessary to do more than to invite him to accept reforms. The upper classes of Europeans are similar to European colonists found elsewhere; with this exception, that they take less patriotic interest in Egyptian affairs than they would if they were living in a colony of their own country. The low-class Europeans, the low-class Greeks or Italians, who form a large percentage of the population, are a very difficult community to deal with, and the Levantine may be regarded as being neither native nor low-class European.

A brief consideration of the Government of Egypt and the laws in force regarding sanitation is necessary. The ostensible Government of Egypt is complex. Egypt is Turkish territory, ruled by His Highness, the Khedive. There is a Council of Ministers, and the interests of European subjects are safeguarded by the Consuls-General and Consuls. Laws which affect natives can be passed by the Council, but important measures which affect Europeans have to be sanctioned by the country or countries concerned.

The British Agent has under him several British officials who are supposed to act as advisers to the native ministers, but who, in reality and quite openly, have charge of the several Government Departments. He is practically the autocrat of the country.

The only laws with which we are immediately concerned are those which govern sanitation, and they are few in number.

The Government is under a difficulty with regard to them because, although they can be enforced with the native, the procedure with Europeans is more difficult owing to the "Capitulations." In the event of epidemics of certain specified diseases, such as plague, cholera, etc., stringent measures can be adopted with reference to native individuals and habitations. In the case of Europeans, however, it is necessary to invoke the co-operation of their Consuls, which involves delay. There is a sort of Factories Act which is applicable to natives and Europeans alike; it necessitates the passing by the Government of the plans of new "unhealthy establishments," and it allows existing ones to be inspected. Unfortunately, this measure is very elaborate, not well suited to the conditions found in Egypt, and usually considered almost to be unworkable by the inadequate staff at the disposal of the Director-General of the Public Health Department. The native and Levantine are so cunning that they succeed frequently—and I speak from personal experience—in evading this law; and the punishments, especially those given in the mixed courts to Europeans, are so lenient that it often pays the individual to ignore the law and suffer the small penalty.

So far as sanitation is concerned, the Capitulations enjoin that no official may enter the house or property of a European subject unless he has obtained permission either from the subject himself or from his Consul. In considering all sanitary reforms this has to be taken into account; the Consuls sometimes, though not often, are unwilling to assist, and officials in Egypt usually complain that the Capitulations obstruct their efforts. In fact, this has become a tradition, and if any new effort fails, the Capitulations are usually blamed. There can be no doubt that the measure is an obstacle, though not such a serious one as is made out, and to this point I shall again refer.

Other questions will be mentioned in the description of the several campaigns, which will now be taken seriatim, in the order in which they were started.

Ismailia.—At the present day Ismailia is a sleepy little town picturesquely situated on the shores of Lake Timsah. De Lesseps intended it to be one of the greatest ports in the world. When the Suez Canal was opened, and the venture an assured success, it was determined to make Ismailia a model city, a thriving port, and the headquarters of the Suez Canal Company. With this end in view, the Company acquired the control of a large piece of land on the northern shore of Lake Timsah, a small sea-water lake through which the Canal passes, placed in the centre of the isthmus of Suez. There the offices of the Company were built, surrounded by the houses of its employees; and the largest shareholder, the Khedive Ismail Pasha, built himself a magnificent palace. Villas were erected overlooking the Canal, and gardens were planned and parks marked out. All the ships in transit of the Canal stopped there, and passengers were to land, and cargo to Egypt was transshipped. Then there was boating, bathing free from sharks, shooting, and every inducement both for work and play. Ismailia, though young, was very healthy, being situated in the desert and isolated from other towns. It was completely under the control of the Company, who spared no expense to make it a model town free from insanitary native huts or unsightly modern flats and hotels. The Government Offices of the district were situated there, with the Courts, and the town soon began to prosper. De Lesseps' intention was on the highroad to fulfilment when, owing to ignorance, a false step was made.

The town was supplied with fresh water by a shallow canal which connected it with the Nile. The canal was built in 1877, and replaced the old system of transport of water by camel, which was in vogue during the early years of the Suez Canal construction. The water in this canal, which was deepened in 1882 to allow the passage of canal boats trading between Cairo and Ismailia, was laden with a rich silt from Abyssinia, and it was used to irrigate the desert and the marked-out parks and

gardens, with the result that a veritable oasis was formed. Trees were planted, and the gardens grew apace under the influence of the irrigation. But the water was not properly controlled, and much of it ran to waste, forming shallow marshes and ponds in and about the town. With the marshes came the mosquitos; and with the mosquitos came the fever; and with the fever came the—downfall. In the days of its salubrity, Ismailia had progressed very rapidly, rising to a population of 10,000; but when the fever appeared, all progress stopped and decadence took its place. Nearly 2,000 cases of malaria were treated every year, but probably many more occurred. Men, both Europeans and natives, were unable to work, children were always ill, the death-rate increased, while the birth-rate fell. Every one was down with fever, and trade was soon at a standstill. The Government offices were closed and were ultimately moved to Port Said; the tribunals were transferred to the neighbouring towns of Zagazig and Mansurah, and the Suez Canal Company were, in 1900, left to face the question whether the town should be abandoned, or whether the disease could be prevented.

In 1877, when malaria first appeared in Ismailia, 300 cases occurred from August to December. The next year there were 400 cases; and the annual sick-rate remained at about this figure until 1885. The disease then became worse, and in 1891 nearly 2,500 cases were recorded. Acting on the old assumption that the disease is caused by a miasm, the Canal Company made several efforts to combat it by partially draining the marshes, and probably these efforts prevented the epidemic reaching extreme dimensions. The discovery of the transmission of malaria by mosquitos was not completed until 1899. In 1902 the Company resolved to take further advice. They sent a medical officer to Italy to make enquiries about the anti-mosquito measures being conducted there, and then they invited my brother, Major Ross, to go to Ismailia to advise them. He arrived at the Suez Canal on 12th September 1902,

in company with Sir William MacGregor, the Governor of Lagos, who wished to study the malaria at Ismailia with him.

The first measure to be recommended by Major Ross was the extermination of mosquitos, which was immediately put into effect, the Company bearing the entire expense. The main breeding-places for mosquitos were found to be shallow irrigation pools and puddles in the gardens and yards, where Anopheline larvae were taken. Each house has its cesspool, according to the general rule in Egypt, for there are no properly instituted sewerage arrangements in the country, and all sewage passes directly into the "fosse" under the house. The fresh-water canal, which brings drinking - water from the Nile and which passes through the town, does not harbour mosquitos. There the water is running, and fish are plentiful; but a short distance away from the town a marsh was formed by leakage from the canal, and here larvae were found. Rigorous measures were adopted; the marsh was drained and the pools were filled up. A mosquito brigade was formed, which consisted of a European foreman and two natives. Their duty was to visit every house once a week, and to treat the cesspools with petroleum. Irrigation canals and channels were cleared of reeds, and the water made to flow swiftly. When a certain garden had received its proper supply of water, the flow was stopped and the water allowed to soak in. All the work was systematic; each breeding-place was visited and treated on a certain day in each week, which was kept up throughout the year. Similarly, all water vessels, tubs, and flower vases were emptied. A penalty was imposed on the inhabitants if they did not report untreated collections of water to the authorities. No expense was spared to make the new sanitary measure a success.

There is one difference between the Ismailia campaign and the others which must be mentioned. The Capitulations did not form an obstacle there. The laws at Ismailia were the same as those elsewhere, but the Canal Company made short work of the Capitulations, for practically all the inhabitants, whether

European or native, were employees of, or dependent on the Company, and if they objected to the visits of the brigade, they could be either dismissed or have their salaries cut.

Instituted by their President, Prince d'Arenberg, the Suez Canal Company can well be proud of the result of the anti-mosquito work at Ismailia. Last year not a single new case of malaria occurred there, and the disease has been stamped out.

Ismailia affords a striking example of the great commercial importance of malaria prevention. When De Lesseps decided that the headquarters of the Canal should be at Ismailia, he, of course, did not appreciate the possibility of invasion by malaria. I believe that his decision was due to the presence of the large natural harbour afforded by Lake Timsah, where a fleet of ships could anchor to take in coal and provisions on their way through the Canal, without obstructing the transit of other ships. It is true that all coal has to be imported from Europe, but in spite of the fact that Ismailia is nearly 50 miles from the Mediterranean, the presence of the natural harbour there was so important, that the extra expense which would have to be incurred in bringing the coal through the 50 miles of canal was hardly taken into consideration. In fact it paid the Canal Company to insist on all coal being brought to Ismailia, because dues were obtained for its transit. Commercially, everything was in favour of Ismailia being made the headquarters of the Canal. The point is an interesting one, for De Lesseps' great commercial proposal was disposed of by malaria. For years the Company tried to force ships to coal and land passengers at the fever-stricken town, but now they have been compelled to relinquish the natural harbour, and to build an artificial one at Port Said (which requires, and always will require, incessant dredging) at an outlay of many millions. Even natural assets of enormous commercial value, such as this harbour in the greatest shipping highway of the world, fail utterly in the face of a comparatively small epidemic of malaria. The artificial harbour at Port Said has been built, and Ismailia has dwindled

into insignificance, at a cost which probably can never be estimated.

The results of the anti-malaria work are well known. Accurate figures, showing the number of severe cases of malaria which had occurred before the mosquito work was started, had been kept, and these could be compared with those obtained afterwards. Every employee of the Company is obliged to go to a doctor when ill in order to obtain a certificate of absence, so that all cases were easily recorded. But there must have been a number of cases of early and slight fever, in which the malaria parasite would not be in evidence, and which would not necessitate the patient lying up; and besides, there were those cases which occurred in persons who were not employed by the Company, and also many native women and children, who will not go to doctors. But the results which are given by Dr Pressat are very interesting.

Years.	Cases.	Years.	Cases.	Years.	Cases.
1877	300	1888	1,400	1899	1,545
1878	400	1889	1,450	1900	2,284
1879	500	1890	1,900	1901	1,990
1880	400	1891	2,590	1902	1,551
1881	450	1892	2,050	1903	214
1882	480	1893	1,750	1904	90
1883	550	1894	1,100	1905	37
1884	900	1895	1,350	1906	No fresh cases.
1885	2,000	1896	1,150	1907	No fresh cases.
1886	2,300	1897	2,089	1908	No malaria contracted in Ismailia.
1887	1,800	1898	1,545		

And now, although cases of malaria are occasionally introduced into the town from the villages situated higher up the fresh-water canal, such as Mahsammah, Nefiche, and Tel el Kebir, yet the disease never spreads there because the Anophelines are absent, and there is no longer any necessity for taking quinine.

The chief difficulty and source of expense at Ismailia was that of the initial control of the irrigation, and the drainage and

filling up of the marshes which surrounded the town. And even now, in the autumn, mosquitos find their way there from marshes situated to the west above the town, there being no habitations between. These marshes would cost much to drain or fill—an unnecessary expense. The number of mosquitos introduced into the town from this source is so small that it may well be ignored, and there is but little chance of their becoming infected, as there are now no cases of fever in the town.

The cost of the permanent works, including the filling up with sand and the drainage of the marshes, pools, and the control of the irrigation, was 50,000 francs (£2,000).

The annual cost of the regular work of clearing streams, ponds, and of keeping the gardens clear of mosquito larvae is 7,800 francs. And the oiling of the cesspools and the general work of the mosquito brigade is 10,500 francs. Of this sum, 5,300 francs are spent annually on oil, and 5,200 francs on salaries and other incidental expenses. Thus the cost of the regular annual work is now 1 franc 90 centimes per head of population per year; at first it was 2 francs 30 centimes. The total annual cost is now about £1,000 per year. And for this sum, after an original outlay of £2,000, Ismailia is kept healthy and free from fever.

But the work must always be kept up, though the cost of it will gradually decrease. As in Port Said, if the mosquito brigade stops work for a week, the mosquitos return. The men require constant supervision. M. Doyen, of the Suez Canal Company, is to be heartily congratulated on the way the work progresses, for the town continues free from malaria.

But Ismailia has never recovered the blow that malaria dealt it, for natives now call it "El turba e'nadeefa" (the clean tomb).

Port Said.—Port Said was a much more difficult problem than Ismailia. The town is larger, with a very cosmopolitan population of 50,000 inhabitants. It is built on a triangular spit of land bordered by the Mediterranean, the Suez Canal, and the shallow sea-water lake of Menzalah. The price of

land is considerable, as the town cannot expand, with the result that high buildings containing residential flats are being erected everywhere. The soil consists of sand, and the subsoil water, which is really sea-water, can be found at a depth of about a metre. Port Said may be said to be built on a sand-bank.

All the drainage from the houses passes into cesspools usually placed under the floor. Owing to the height of the subsoil water, these cesspools are frequently too small to be adequate, and overflow as a common result. They are continually being emptied at great expense, which falls on the landlords, who are not too willing to carry out this duty. It may be imagined, therefore, that before the mosquito campaign was started, that is, before any proper systematic sanitary administration was instituted, Port Said was one of the most insanitary places. Many of the houses had basements, and the cesspools were bricked reservoirs built in among the rooms contained in them. When the cesspools became full, they frequently burst, and the sewage passed into the rooms which formed the basement. I have often seen shops and dwellings with only a wooden flooring separating the living rooms from the sewage, and more than occasionally one used to see persons living in a room with the sewage oozing up through the floor. Without exaggeration, one can say that a few years ago Port Said was famed for being one of the most disgusting "holes" on the face of the earth. Crime was rife, every one looked ill, and it was openly said that all the scoundrels in the world collected there. In fact, little more than ten years ago it was hardly safe to walk in the streets at night. The population is largely European, to a far greater proportion than in most other Egyptian cities; and owing to the Capitulations, the Government police officials could not enter the European houses, and knew little of what was going on.

But the filth and general insanitation were almost indescribable. Sewage used frequently to be seen trickling down the pavements from the overflowing cesspools, and puddles of

faecal matter formed in the roads. Sometimes the stench was such that certain houses and corners were deliberately avoided by pedestrians. Mosquitos bred in the sewage and then bit the inhabitants. The authorities could do little or nothing owing to the Capitulations (so they said). Consuls would be approached by means of endless correspondence and red tape. Sometimes a little improvement was effected, but usually nothing was done. Fever was rife and mortality very high. It was dangerous then, as in Cairo now, to keep a European child in Port Said during the summer months. I have seen an infant's face so covered with mosquito bites, that I thought it was suffering from confluent small-pox. The whole place was a "sink of iniquity," a statement which, I think, will be confirmed by any person who passed through the Suez Canal a few years ago.

But the mosquito campaign has changed all that. Practically speaking, the Capitulations have vanished. Sanitary inspectors, on the plea of mosquito destruction, now enter every house, and every dwelling-room if necessary, in the town. Everything is known and reported to the police should occasion require it. And the remarkable thing is that no one appreciates this reform more than the Europeans themselves, even the lowest class Levantine. Children are healthy, trade has increased, and crime and vice have been reduced to a minimum. In fact, Port Said now resembles a quiet seaside watering-place.

The history of the reform—and it is well worth recording—is a remarkable one. Towards the end of 1905 my brother, E. H. Ross, then a surgeon in the British Navy, was appointed Medical Officer of Health to the town by Sir Horace Pinching, the Director-General of the Public Health Department. The new officer, appreciating the results obtained at Ismailia, owing to the advice of our brother, R. Ross, saw that similar measures would effect far greater results at Port Said. It was true that there was no epidemic of malaria as at Ismailia, but Anophelines had been found in many places, and malaria did undoubtedly occur. *Cellia pharoensis* and *Anopheles maculi-*

pennis, both malaria-propagating mosquitos, were found breeding in puddles and cesspools. Moreover, the ubiquitous *Culex fatigans* and *Stegomyia fasciata* were a positive nuisance. Every one was bitten day and night. The cesspools were full of larvae. E. H. Ross reported as follows :—

"In Port Said, only those who lived in the town before 1906 can realise the misery caused by this pest. Quite apart from the fever, the constant irritation of the mosquito bites, and the persistent manner with which the insects used to attack one, both during the day and night, nearly drove one mad. I have seen clerks in offices arranging mosquito-nets over their desks in the daytime, hanging them on to the gas brackets, and the native workmen in laundries covering their naked feet and legs in paper to prevent themselves being bitten. A certain amount of immunity against the irritation is produced in time, and certain individuals feel the effects of the venom less than others, but they are bitten for all that. Then the heat of the mosquito-net at night. Every time one moved in one's sleep, and threw off any bed-clothes that the heat would allow, one was liable to touch the net with a hand, arm or leg, and to be bitten through the net. And the persistent scratching and the disturbed sleep—these things have to be experienced to be realised. I have estimated that a mosquito-net makes a difference of three degrees Fahrenheit when there is any wind, for it keeps out any air that there is. How well I remember the many evenings sitting at the laboratory table, trying to dissect mosquitos, trying to investigate their interior economy, while they in their turn were doing their utmost to investigate me, nibbling my ankles, raising wheals on my wrists and neck, and making all work impossible. At all events I think that they had the best of the bargain. And then, how often in a fit of rage one has chased them with a fly-wisk, or stalked them with a candle, making oneself hot with the exertion of numberless futile attempts, until driven to bed and the refuge of the mosquito-net, hot and in a bad temper."

It was clear that, in spite of the possibility of there not being very much malaria at Port Said, the general prevalence of mosquitos probably caused illness, and my brother grasped that an attempt at their destruction could only bring good results and might lead to the general cleaning up of the town.

He therefore made enquiries as to how much fever there was which could be attributed to mosquitos. Formerly, a fever of mild type, which used to affect whole households and which lasted for two or three days, was common in Port Said; sometimes it was called dengue and sometimes influenza. Quinine was beneficial in some cases. Malaria did occur, a fact which was proved by Dr Cuffey, the Medical Officer of the English Hospital. Anophelines were sought for and found, but not in very great numbers, although they were breeding in many parts of the town. An examination of 480 children showed that 162 were suffering from anaemia and splenic enlargement, and, out of 136 children examined in the hospital wards, 10 were found with malarial cachexia.

Dengue was also prevalent, and came in epidemics from other parts of Egypt. This disease is said to be conveyed by *Culex fatigans*, which is a very common mosquito throughout Egypt. Then there was the disease known as Simple Continued Fever, which seems to infect schools and factories.

Unfortunately, there were no reliable statistics. Diseases are supposed to be notified in Egypt, but doctors do not obey the regulation, which is not properly enforced. Natives do not usually consult doctors, and patients are frequently not seen until death. The Public Health Department thinks that a cursory examination of a dead body, with a rough speculation of the disease from which the person suffered, is sufficient to form an idea of the diseases prevalent in Egypt. Statistics are published, but they are quite worthless; the only reliable data, therefore, are the number of registered deaths. In 1904 the number was 1,450; in 1905, 1,208; in 1906, 1,199; and in 1907, 1,412. The actual population was unknown, and although a census has been taken recently, it is not accurate. The absence of reliable figures is the most unsatisfactory part of anti-mosquito work in Egypt, except at Ismailia.

As in subsequent campaigns, two steps were necessary at the outset: to estimate the cost and obtain the necessary funds,

and to arrange a scheme to overcome the Capitulations. To estimate the cost a house-to-house examination was made. At first it was decided to treat only the European quarter, which lies to the east of, and is distinct from, the native quarter. The European quarter was chosen because it contained more mosquitos; and if the work there was successful it would be a good example to the natives who, especially the Mahomedans, are suspicious of innovations. Moreover, this was the first campaign in which the Capitulations formed a difficulty, and the starting of the campaign in the European quarter formed an experiment which demonstrated how effectually that difficulty could be overcome. I wish to lay stress upon this point, for the Capitulations in Egypt have defied the greatest administrators, yet a junior official in the Egyptian Public Health Department succeeded, by instituting the mosquito campaign at Port Said, in showing how easily this obstacle to reform can be overcome. My brother reports as follows:—

"Beginning at the easternmost end, namely, at the street bordering the canal, every house and garden in the European quarter was visited seriatim. Street by street the examination was made, and the number of permanent water collections, such as cesspools, flooded cellars, fountains, water tubs, counted, with a note as to the number of square metres water surface they contained. The number of days required to make this examination from east to west and from north to south by one person was also noted, and then an allowance of half a litre of mixed equal parts of crude and refined petroleum allowed for each square metre of water surface per week. This gave the amount of oil required to destroy the mosquito larvae in all the water in the town every week, and the estimate was therefore multiplied by fifty-two, as the work has to be repeated every week during the year, some mosquitos breeding during the winter as well as during the summer months. Then by a simple calculation it was determined that it would require a gang of workmen, containing five men under the charge of a foreman, to apply the oil to all permanent collections of water in the European quarter every week; and therefore one mosquito brigade would be required to get rid of the mosquitos in this part of the town. The price of oil in Port Said is as follows:—

crude oil in tins containing 18 litres (4 gallons) is 8 piastres per tin, and refined oil is 11 piastres per tin. To the cost of the amount of oil required per year, then, it was necessary to add the yearly wages of the five workmen at 6 piastres per day each (1s. 3d.), and the foreman at L.E. 5 per month. The purchase of an oil barrel on wheels, five buckets, five half-litre tin measures, overalls for each workman, and allowing for small repairs to the equipment, etc., the estimate of the cost of the campaign in the European quarter was obtained per year. The estimate amounted to L.E. 800, but the actual cost for the first year was L.E. 719 (L.E. = about 21 shillings).

"The estimate had been made and the money subscribed, and it remained only to obtain the necessary official permission to enter the houses of all foreign subjects from the various consuls, so that the work would not be hindered by the domicile clause of the Capitulations. In order to obtain this permission, we advertised in every direction, spoke to all influential persons and made people interested, and then when the Governor wrote officially to all the consuls, they unanimously consented to assist us in every way, and gave the sanitary authority of the town permission to enter the houses of their subjects for the purpose of 'disinfection and for killing mosquitos'; and each consul appointed a prominent member of his community, to whom any difficulty could be referred without officially disturbing the consulate concerned. We, on our part, undertook to supervise the work carefully, as the workmen had to enter every house once every week, and to take all reasonable precautions to ensure their honesty. And in this respect it is noteworthy that although the work has now been in progress without interruption for three years, there has not been a single charge of dishonesty upheld against the workmen, nor has there ever been occasion to appeal to the consular delegates.

"The preliminaries had taken two months, but everything was ready for a start early in May 1906. Many good authorities had said that it would be found impossible to clear the town, or a portion of it, of the mosquitos which infested it; even as they say that it will be impossible to clear Cairo to-day. But it was determined to try. A gang of five men were engaged. They were all natives of good character who lived in and had families in the town. A Greek foreman was found who could speak all the necessary languages, which include Arabic, English, French, Italian, Greek and Turkish. An iron barrel, capable of holding 300 litres, fitted to wheels to enable it to be pushed about the streets, was bought, and each workman was given a bucket, a half-litre measure, and a broom-stick,

while the foreman was armed with a note-book. This was the equipment for the European quarter ; and the following routine was instituted. The quarter was subdivided into six equal portions, one for each week-day. Work begins every Monday morning at the Governor's house, and one member of the brigade visits every house allotted to him that day. Every house is numbered, and is placed in the charge of one workman, who is responsible for the mosquitos, or the absence of them, in that house. On Tuesday morning at seven o'clock the brigade begins where it left off on the Monday evening ; and so on, until the whole of this part of the town is completed by Saturday night at the Governor's house again. Thus every house in the quarter is visited by the same workmen at the same hour of the same day every week, and the tenants know when to expect him, and can complain to the foreman if he doesn't come regularly. Each workman fills his bucket with mixed crude and refined oil from the oil-cart in the street. When he arrives at the house allotted to him he asks the tenants if they have been annoyed by mosquitos during the week. If the answer is in the negative, he enters the house with their permission, and pours a small quantity of oil down each water-closet and sink. He mounts to the roof, where in Port Said the wash-houses are usually situated, and empties out all tubs and basins containing water. He examines the kitchens and servants' rooms in each flat, for any collection of water which they may contain. Householders are now quite used to this weekly visit, for the men are well known, and it serves to remind the tenants to see that the workman carries out his duties properly ; and at the same time they are advised to empty out all water in which mosquitos are likely to breed, as in flower-vases and in suchlike utensils. The workman then descends to the basement, where he examines carefully every cellar for any water or sewage, and he then opens the cesspool of the building and pours into it that amount of oil, allowing now, after three years, a quarter of a litre for each square metre of water surface it contains, as nearly as possible. The oil on the surface of the cesspool water is then well stirred with the broom-stick, so that it may spread everywhere evenly. Lastly, he visits the garden and examines the fountain and the water-taps, flower-pots, and the yard if there is one ; and then, when finished, he goes on to the next house allotted to him, repeating the process while refilling his bucket from the oil-cart in the street.[1]

[1] In Port Said, mosquitos do not breed in the drinking-water cisterns, as the filtered water contains no food for the larvae. We have therefore been able to ignore house cisterns.

"But should the tenants complain of the presence of mosquitos, and the workman find that the cesspool is full, or that the cellars contain water, he at once informs the foreman, and they both begin a careful search for the mosquito larvae. The foreman reports at the Public Health Office every morning at noon, and details the presence of mosquitos, and whether the larvae have been found or not. He also reports on the sanitary condition of the streets dealt with, and the houses examined that day, and states whether any cesspool requires pumping out, or whether any cellar has water in it, in which case the proprietor of the house is dealt with by the Sanitary Inspector, and an extra foreman is sent down to find the mosquito larvae, when they cannot be found by the brigade.

"Ultimately, when the mosquito larvae have been found—and they can always be discovered if carefully sought after, either in the house itself or on the premises—some are put into a bottle and examined. If there are any nymphs, it is known that the eggs have been laid for more than seven days, and therefore the workman responsible for the house has neglected to oil the water in which they were found, and he is punished. But more often it is found that the proprietor of the house is at fault, and the mosquitos have reappeared because he has neglected to have the cesspool pumped out; and in consequence of its being full it has overflowed or has burst, and the sewage has collected in the cellars, and there the mosquitos were breeding. In this case the proprietor is written to and is warned to empty the cesspool without delay, or he will be sued and fined in the Courts; for there is a law which enables us to do this. Every cesspool in the town has been fitted with a movable iron door, so that access can be obtained to its interior when desired. We persuaded the proprietors of all the houses to put these in, for we found that there are some cesspools into the farthest corners of which the oil cannot reach when poured in through the water-closets and sinks, or even through the door. This is probably due to solid sewage floating on the surface of the water and preventing the oil spreading evenly. In such places the oil is well stirred with a broomstick to make it reach all parts; but if this fails also, then we have had recourse to the automatic oilers described in the *Annals of Tropical Medicine* for June 1907, and they have been most successful for the purpose, though even when these are in use the cesspools containing them must be carefully examined every week in case the oiler becomes clogged. This is recognised by the presence of male and female mosquitos in the cesspool after ten days have passed since the clogging.

"This is the routine which has been followed ever since its institution in the European quarter, and which has been since copied exactly in the native quarter. It has undergone no modification, except that the amount of oil used weekly has now been reduced considerably.

"The work in the European quarter was begun in the first week in May 1906, and by the end of July, the mosquitos in this part of the town were sensibly diminished. The summer was very hot and damp, but we were able to rest in the afternoons without being bitten by *Stegomyia.* By the end of August mosquitos had been reduced greatly; and in many parts of the European quarter mosquito-nets were no longer required. But it was found that we were using an enormous amount of oil, nearly 3,000 gallons of equal parts crude and refined oil per month. This was because of the sewage in the cellars. It was therefore determined to try to reduce the amount of water in these cellars permanently, so as to economise the oil. There were then 400 cellars in the town flooded with sewage, having a total of 7,296 square metres water surface.

"This condition of insanitation was due to the following peculiarity of the place. The subsoil water (sea-water) is only a metre below the ground-level, as the whole spit of sand, on which Port Said is built, is reclaimed from the sea. Owing to the small area reclaimed, the ground-rent is very high, and in consequence many large buildings, of often four and five and even six stories, have been erected, and divided into flats and suites of offices. These houses have been built with basement cellars. In every building the portion of the cellars situated under the main entrance hall has been shut off by brick walls from the cellars to form the cesspool, into which all the house drains discharge. These cesspools were supposed to be sealed except for a ventilating shaft, but were, in the majority of instances, badly built and leaky. The result was that the sewage was passing into the adjoining basement cellars as fast as it poured into the cesspool. Once in the cellars it did not rapidly soak into the sand forming their floor, as at that level the sand was already saturated with the subsoil water. So that we found when we began our campaign, that in the 400 large buildings the whole basement constituted the cesspools, and in them mosquitos, flies, cockroaches, and other insects were breeding in myriads. These cellars were also swarming with rats and were the cause of the perpetual smell of sewage which assailed one even in the street."

Towards the end of 1906, people in the European quarter

were beginning to discard their mosquito curtains and life was becoming more bearable, and, as was anticipated, the natives began to ask why their quarter was not treated also. In response to their request, therefore, a campaign was started in the native quarter in January 1907. A fresh brigade was formed, and the campaign was conducted on precisely similar lines to that in the European quarter.

The results at Port Said have fully compensated for the cost of the undertaking. When I was in Port Said for several months in the spring of 1908, I never saw a single mosquito—a marvellous contrast to my experience in the same town ten years before. Mosquito curtains are never used now by the inhabitants, and no precautions whatever are required to be taken against being bitten.[1]

The following table gives the cost of the campaign, the Egyptian Pound being equivalent to about twenty-one shillings.

Year.	Amount of oil used.	Total cost.
1906 European quarter only, May to December . . .	14,296 gallons.	L.E.384·768
1907 European and native quarters for the whole year . .	41,120 gallons.	L.E.1,176·275
1908 Both quarters. 12 months. Cellars filled up . .	31,670 gallons.	L.E.1,023·463

It will be noticed that the amount of oil used, and therefore the cost of the work, diminishes every year, and this should be expected. Should the present cesspool system of sewage disposal ever be replaced by drainage, the amount of oil required will become very small, and the size of the gangs could be reduced, for there would be less work to be done.

But the cost of the campaign has been trifling compared with the beneficial results to the community. Everybody has been delighted with the results, fevers have declined, and the children have become healthy. The streets no longer smell as they did, and the place has improved in every way. The

[1] In 1902 Sir W. MacGregor and I were "devoured alive" by *Stegomyia* in Port Said. During three visits in 1907 and 1908 I did not see a single mosquito.—R. Ross.

absolute disappearance of mosquitos is a most remarkable thing, and is probably unique in history; yet if the brigades become slack, the gnats reappear in a few days. The inhabitants now complain at once if mosquitos appear, and their breeding-places are then looked for. They will always be found in the immediate neighbourhood.

But the general effect on the town is the most striking example of the efficacy of the sanitary measure. Officials of the Government enter every house unmolested; they know all that is going on, and, in consequence, vice has been greatly reduced. Port Said is no longer a "sink of iniquity." The streets are safe; there is less prostitution there than in any town in Egypt; gambling dens have disappeared; all is known in spite of the Capitulations. If a cesspool bursts or overflows, the landlord has to remedy the defect, or he has to stand the abuse of his tenants owing to the return of the mosquitos. Moreover, he is promptly sued by the Government. The consuls themselves, far from obstructing, now assist the authorities in every way.

It is right to mention that the Suez Canal Company, through its President, Prince d'Arenberg, have placed a large sum of money for the maintenance of this campaign at the disposal of the Egyptian Government.

This campaign at Port Said is perhaps the most successful which has ever been started, and any one who is able to compare Port Said in the old days with the present state of affairs will, I am sure, eulogise the work which has been done. A great deal remains to be done, however; but the mosquito campaign has paved the way for it, and has shown that with a little perseverance sanitary reform in Egypt is not nearly so difficult as is usually made out.

There can be no question that drainage is the next thing required at Port Said. The present cesspool system is quite inadequate, and it should be replaced by proper sewerage as quickly as possible. A scheme has been drawn up and passed,

I believe; and I have heard that the Suez Canal Company have offered to advance the necessary funds for it. It is difficult to appreciate, therefore, why it has not been undertaken. The Egyptian Government itself appears to be doing very little for Port Said, but the least it might do is to accept the Canal Company's offer and drain the town. At present negotiations are on foot between the Government and the Company regarding the latter's concession, and I suppose that the health of Port Said has suffered for this delay. The drainage will be expensive, for it will have to be done on the artificial pumping system, there being no "fall" at Port Said. Still, drainage there is almost more necessary than at Cairo. If the place were properly drained, the expenses attached to the mosquito campaign would be reduced almost to a negligible quantity.

Cairo.—Cairo is a very much larger city than Port Said. It contains an estimated population of more than half a million, the European element being smaller than that of either Port Said or Alexandria. The town is situated on the east bank of the Nile, at the apex of the delta of the river. Facing Cairo, on the other side of the river, there is the town of Ghizeh, and between the two, on an island, there is the residential suburb of Ghezireh. Cairo proper can almost be divided into two portions—the native to the east and the European to the west—by the old canal, now filled in to form a tramway, called the Khalig. Suburbs extend to the north and south, but the city itself is compact, and, for its population, does not cover a very great area. Owing to the fact that the Government owns most of the surrounding land and will not part with it cheaply, the price of land is exorbitant, and in consequence there is great overcrowding, with the erection of high buildings containing residential flats (in this city, with its almost tropical climate).

Cairo is infested with mosquitos. To destroy them throughout the city would be a stupendous, but not impossible, task, and it would involve an expenditure of about £20,000 annually.

It is true that in the winter months the gnats are not so numerous, but in the summer and autumn the pest is as bad as in many tropical places.

Like Port Said, Cairo is built on a lake of subsoil water. The Nile water, of course, is not confined between the banks of the river. In fact, the underground river is much larger than the Nile itself, and it flows directly beneath the city of Cairo. The height of the subsoil water, therefore, varies with the height of the Nile, and in the months of September and October, when the Nile at Cairo is in flood, it frequently happens that the basements of the houses are filled with water.

Every house, native or European, has its cesspool, which leads directly into the subsoil water; and for centuries past all the sewage of the city has passed directly into this underground lake and has saturated the soil all round. The cesspools of the houses built by Europeans are called "percolating pits." They are commonly built immediately beneath the houses, and they vary in size according to the uses put to them. Their bases are deficient in bricks and are formed by the porous earth only. Every cesspool dips into the subsoil water, and bricks are removed from the walls in places to facilitate the percolation of the sewage into the surrounding earth. It can veritably be said that the land on which Cairo is built is sodden with sewage. Nearly all the pits are ventilated by short intake and long uptake shafts carried up to the roofs of the houses, and it is by means of these pipes that mosquitos gain access to the cesspools. A few of the more modern houses are fitted with "fosses morass," which are, in reality, modified liquefying tanks. Some of them are made on a fairly satisfactory basis, but usually they are pretences, and are nothing more than rather elaborate percolating pits.

The native cesspool is, if possible, more insanitary. It consists of a long trough dug in the earth under the house. Stone flags form its roof, and one of these flags, having a hole

in it, is used as the closet. No water flushing arrangement is employed, and fluid soon soaks into the earth. For this reason mosquitos do not breed so readily in the native as in the European cesspools, and consequently they are not so numerous in the native as in the European quarter of the city.

Both classes of cesspool have to be pumped out from time to time, and conservancy carts form an important, if not an ornamental, portion of the Cairo traffic. The pumping out process is an expensive item for the landlord, and is a lucrative business conducted, not by the Government, but by private enterprise. On the plea of expense landlords frequently neglect to cause the cesspools to be emptied until the last moment. At present there are no drains. For the last three years the Government has made a special drainage department for the construction of sewerage. I have heard that the plans of the scheme have already been passed. In the meantime, when the soil is turned up in any part of the town for the purpose of making the foundations of new buildings, the stench is usually indescribable, for the subsoil water is soon reached, and that water is practically sewage. Moreover, these excavations form fruitful breeding-places for mosquitos.

The water-supply of Cairo is derived from shallow wells placed below the town, a source which has given rise to considerable controversy. In the native quarters, almost every house has its own well, which is bored close beside the porous cesspool. The water in the well is that which has usually percolated in from the neighbouring cesspit. The natives prefer this or the Nile water, which, of course, is polluted, to that supplied from the wells at Rod el Farag. When cholera broke out some years ago, many of the wells were filled up, but they have nearly all been reopened. In all the cesspools, wells and other collections of water which riddle the city, mosquitos, cockroaches, and all sorts of vermin thrive. The human mortality is enormous, especially the infantile mortality. The figures supplied by the Public Health Department are

unreliable (as I know, for I have assisted to compile some of them). The actual population is unknown, many deaths are probably never reported, and sickness is not usually notified. All deaths are supposed to be registered, the diagnosis usually being made by a brief inspection of the dead body. Doctors will not notify disease, because they say that it ruins their practice. Landlords prefer to knock holes in the sides of their cesspools and allow the sewage to flood their cellars and basements rather than go to the expense of having them pumped out. The water-supply is not the best obtainable, the streets are not properly cleaned, and enormous heaps of dung and rubbish have been allowed to accumulate for years past on the outskirts of the city. Many of the streets are not metalled; which perhaps is a good thing, for the rough surface acts as a sponge for the stale urine which would otherwise collect in puddles. Dung and street refuse are used as fuel generally; and large collections of this rubbish are kept for this purpose on the roofs of the "Turkish Baths." Nearly all the dogs have been destroyed owing to outbreaks of rabies, and mangy cats have taken their place as natural scavengers. Hordes of flies, which breed in the dung and rubbish, abound everywhere, and are nearly as great a nuisance as the mosquitos. Without exaggeration, Cairo may be described as a city which is hardly fit for habitation, and at present it must rank with Moscow, Pekin and Hankow as being one of the most insanitary spots in the world. Since the mosquito campaign, Port Said is a health resort compared with it.

The common mosquitos found are *Culex fatigans*, *Stegomyia fasciata*, and a species of Anopheline called *Cellia pharoensis.* They breed in the cesspools, wells and ornamental fountains. The natives love fountains, and their houses always contain large numbers of water vessels of all sorts and sizes.

Fevers are undoubtedly rife. Typhoid broke out in one or more of the principal hotels each season when I was in Cairo; for it must be remembered that Cairo is a favourite

resort in the winter. The typhoid is probably propagated by the vegetables, which are usually washed in old canals, or even cesspools, by the natives when they bring them into the town. Some years ago a scheme was made to prevent this washing, which is done to make the vegetables glisten and look fresh for the market; but the Government would not adopt it. Dengue, which is probably propagated by *Culex fatigans*, occurs in epidemic form every year. Malaria exists in Cairo, but it is only of the mild tertian and quartern types. Following on some questions which were asked in the House of Commons last year, a controversy arose in the Press in Egypt regarding the incidence of the disease. One paper said that it did not exist. But it does. Only last summer an outbreak occurred at Ghezireh, and according to the Statistical Report of the Public Health Department for 1908, twenty-nine deaths occurred in Cairo alone from the disease. The Senior Medical Officer of the Egyptian Army reported an outbreak at Zeitoun and Abbassieh (suburbs of Cairo) on 3rd November 1907 (Letter No. 411). I have myself seen cases of malaria, and have taken Anophelines frequently, especially at Ghezireh. Probably the incidence of the disease is far greater than is commonly supposed. Several other diseases, such as Simple Continued Fever, are confounded with it. Elephantiasis, also conveyed by Culicines, is a common complaint, and victims are frequently seen begging in the streets.

A real estimate of the incidence of malaria has never been made. Last year a small spleen census was taken among some children, but the result was vitiated by the discovery of a disease, provisionally called "Endemic Cirrhosis of the Liver," which occurs among them. Still, malaria exists, and other fevers too; which is sufficient ground for the institution of a campaign, apart from the other great sanitary benefits conferred by these undertakings. At one of the campaigns, also, that at the Police School, figures have been obtained,

which I shall quote later, and they demonstrate amply the beneficial results which would accrue from general mosquito reduction. Cairo is visited annually by thousands of English and American tourists, who live in palatial hotels, and who cannot realise the real state of affairs. From a superficial visit, Cairo is a most picturesque city; but let them go off the beaten tracks into the depths of Boulac, Saida Zenab, and Bab el Sharia, districts which visitors do not often see, and they will then appreciate the insanitary precipice whose brink they visit.

The first mosquito campaign in Cairo was started by a few English residents in the district of Kasr el Dubarra in 1904 or 1905. Only a few houses were treated (by the residents themselves), and although a reduction in the number of mosquitos resulted, the campaign soon lapsed owing to the fact that many of the breeding-places were overlooked. In the meantime, Sir Horace Pinching, K.C.M.G., Director-General of the Egyptian Public Health Department, who was very pleased with the work of my brother, E. H. Ross, at Port Said, gave me an appointment as probationer in that Service, for which I abandoned the Medical Service of the British Navy—in the hope of having better opportunities for scientific and sanitary work. In the autumn of 1906, His Excellency Mansfield Pacha, then the Commandant of the Cairo City Police, suggested to me that an experimental campaign should be instituted privately in the Muski district near the police headquarters. Most of the necessary funds were provided by the police, and Mansfield Pacha used his influence to get subscriptions from the other residents whose property was to be treated. The campaign included the building of the Mixed Tribunals, the Opera House and the Esbekieh Gardens. This campaign is an interesting one, for the whole area involved was very small, about half a square mile of densely-populated buildings situated right in the heart of the city, where the houses were, if possible, more infested by

mosquitos than elsewhere. The result was remarkable, for in three months the mosquitos had practically disappeared from the area treated, whereas all round they remained as bad as ever. Every cesspool was oiled once a week with a mixture of refined and crude petroleum in equal parts. All water vessels were overturned and dried out. In the Opera House the audience could watch the performance in comfort owing to the mosquito campaign, whereas if any one crossed the street to one of the uncampaigned houses, he would be pestered by hundreds of insects. This campaign proved the point that mosquitos will not migrate far from their original breeding - places. We received no opposition whatever from the inhabitants, in spite of the fact that the majority of them were Europeans, and incidentally hasheesh and gambling dens were discovered and reported to the police.

Early in 1907 a new campaign was started in Kasr el Dubarra by myself under the orders of Sir H. Pinching. This district is bordered by the Nile and contains the best residential houses in Cairo, including the British Agency. It is right to mention that this campaign was restarted largely at the instigation of Lady Cromer, who suggested that it might be instituted on scientific lines by a responsible official; not by private enterprise, like the first attempt in 1904. Sir Horace Pinching ordered the funds to be provided after a rough estimate of the cost had been prepared. The next step was to obtain permission from the residents themselves to allow us to enter their houses. It was considered preferable to obtain permission from individuals rather than from their consuls, because at that time some of the consuls in Cairo were not on very friendly terms with the Government. Letters printed in several languages were sent to the residents, and a large number were soon answered giving permission. The European residents never appeared to object when they appreciated that they would have nothing to pay—a point which was clearly stated in the printed letters. Curiously

enough, the only persons who objected were the natives who had no right to refuse. We could, of course, have entered the houses of the local subjects without their permission, but Egypt is governed by a diplomatic administration rather than by a scientific one, and it seemed more diplomatic to treat both native and European alike. The objectors were called upon and the campaign explained to them. Lady Cromer came to the rescue, and was indefatigable in calling and seeing personally many of those who refused to allow us to enter their houses, and allaying their suspicions. By the end of January the campaign was in full swing, the brigade consisting of a European foreman and two native labourers.

In about three months, when the mosquitos ought to have been reappearing owing to the return of the hot weather, the residents noticed that their houses remained practically free, in spite of the fact that the pest was assuming its usual proportions in the other uncampaigned parts of the city. One by one the objectors asked that the campaign might be instituted in their houses, for they began to realise the benefit of it. For instance, one of the gentlemen who objected to the campaign at the outset was a native prince. His palace, therefore, was not entered. After a few months his neighbours began to complain that the mosquitos from his house annoyed them; and he, on entering their houses, remarked on the reduction of the number of gnats. Suddenly a polite letter was received asking for a visit from the brigade, and the letter was followed by a visit from the objector himself, who implored us to destroy his mosquitos. From instances such as this we learnt that it is better to ignore objections to the visits of a brigade rather than to endeavour to persuade people. Let the campaign be continued in the houses of others; after a short time I think that it will be found that all objections will disappear.

The following October, when the mosquitos ought to have been at their worst, Kasr el Dubarra enjoyed comparative

immunity from them, and at the Semiramis Hotel, which is in the district, visitors and tourists could sleep without mosquito curtains, the only place of this nature in Cairo where this has ever been done.

At the beginning of 1907, on the initiative of Sir H. Pinching and Major Elgood, a small campaign was also started by me in the Police School at Cairo. This school contained about five hundred students, recruits for the Police Force in Egypt, and it was established in an old palace in the Abdin quarter of the city. The palace is a very large building surrounded by a garden. It is built on the native plan in the shape of a quadrangle with a large courtyard in the centre. It was infested with mosquitos, from which the students suffered greatly. The ceiling of the school hospital, which formed part of the building, used to be black with the gnats, a sight I have never seen before. Like many of these native palaces, there were more than seventy latrines, all of the native pattern, which communicated with large cesspools under the building. These cesspools were not ventilated, and the only access to them was through the closets themselves which led directly into them. In the garden there were the inevitable fountains, and a sort of pond which was used for irrigation purposes. Oil used to be poured down most of the latrines ; others were sealed up. The pond and fountains were emptied. The number of mosquitos was greatly reduced in a very short time, and before I left Cairo a year later, they had practically disappeared. Major Elgood, the Commandant of the school, then kindly undertook to supervise this campaign, and when, at a later date, the school was moved to new buildings at Abbassieh, a fresh campaign has been started there by him with most successful results. The year before this campaign was started, 26 to 27% of the *personnel* were admitted to hospital suffering from various forms of fever, including dengue and simple continued fever (probably some were cases of malaria). After the campaign had been in progress for some time, the

fever showed a decline, thus—June 1908 to June 1909, the incidence of fever fell to 7·2%. The average number of the *personnel* is about five hundred. In a paper describing this campaign,[1] Major Elgood gives the following figures:—Year 1907-1908, eighty-eight cases of dengue occurred. During the year 1908-1909, when the campaign was in progress, the number of cases fell to six.

In March 1907 a campaign was started at Heluan by myself under the orders of Sir H. Pinching. The initial procedure was similar to that adopted at Kasr el Dubarra. Heluan is a town built in the desert about 15 miles to the south of Cairo. It is famed as a health resort owing to its hot sulphur springs and dry, invigorating climate. There are many hotels and sanatoria, and the town itself is a growing one considerably larger than Ismailia. The reason why Sir H. Pinching ordered the campaign to be started at Heluan was because malaria existed there. Anophelines had been taken, and examination of blood-films taken at random from native children demonstrated benign parasites; an examination which had been made sometime previously by Dr Dreyer of the Public Health Department.

A new brigade was formed, and all the water collections and cesspools were treated with petroleum. Mosquitos were found to be breeding in some pools of sulphur water, which had collected in some places owing to overflow from the sulphur wells. It seems that after this water has been allowed to stand for some time, the sulphur precipitates, and the larvae of mosquitos (including these of *Cellia pharoensis*) will then thrive in it. An attempt was made to drain this surplus water, which will be an expensive undertaking. In the meantime, as much of it as possible was oiled.

Heluan was famed almost as much for its mosquitos as it

[1] "Some Account of the Preventive Measures taken against Mosquitos at the Police School, Cairo," by Major P. G. Elgood. *Cairo Scientific Journal*, No. 34, vol. iii., and *Brit. Med. Journ.*, 16th Oct. 1909.

was for its sulphur baths; even in the winter months one was being bitten night and day. Nearly all the gardens have fountains, but unlike the houses in Cairo, the cesspools are usually to be found in the garden or courtyard. *Stegomyia* breed freely in the fountains, which were therefore stocked with fish. Some householders objected to oil being poured into the fountains because it is unsightly, and it was found that a common variety of Nile fish (*Tilapia nilotica*) is very hardy and will keep small collections of water free from larvae. The cesspools are constructed very deeply in Heluan, as the subsoil water, especially in the east and north of the town, can only be found at a considerable depth. There are not many water-closets, and the result is that some of the cesspools are dry except in "pockets" which have formed in the earth. To reach these "pockets" with oil, it was frequently necessary to send a man down into the cesspool itself. In this case a rule was made that a lighted candle was previously lowered to test for the presence of poisonous gases — an important precaution against accident.

The results of the campaigns in Cairo and Heluan may be summed up together, for in every case it was practically the same. The number of mosquitos began to diminish perceptibly in about three months; and in Heluan, in six months, people were sleeping without mosquito curtains. Even those people who were inclined to doubt the possibility of ridding districts of Cairo of mosquitos, had to admit that the destruction of the pest is not only a possible, but really an easy problem.

In September 1907 Dr Dreyer again examined blood-films from some children in Heluan, and found that very few parasites could now be demonstrated. No cases of dengue occurred there during the autumn of the same year, although an epidemic of the disease was in progress in other parts of Egypt.

Apart from the prevention of disease, however, mosquito campaigns are valuable measures to be adopted in a country like Egypt, for they bring the officials who organise them more

in touch with the inhabitants, who, consisting largely of natives, are inclined to mistrust those set above them. Mosquito brigades are constantly visiting the houses, and when the inhabitants appreciate that something is really being done for them, they take an interest in the work, and ultimately further this and other sanitary reforms. When I left Egypt in January 1908 there was no one who objected to the visits of the mosquito brigades, nor were there any complaints that any member of them had abused the privileges conferred on European subjects by the Capitulations. In fact, the Capitulations put little real difficulty in the way of sanitary reform. Europeans and their consuls are fully alive to the possibilities of scientific measures properly carried out; and if Europeans and even natives are assured that efforts to improve sanitation are made conscientiously, I believe that they will forego, as has been proved by the mosquito campaigns, the one privilege which they prize, namely, an immunity against interference by Government officials. The difficulty caused by the domicile clause of the Capitulations, which is used as a cloak for nearly all administrative sins, vanishes in the face of a mosquito campaign. The experience gained in Cairo and Heluan proves conclusively that mosquito destruction is a desirable measure to be adopted in Egypt, and it is remarkable that this fact was not appreciated before.

Unfortunately, Sir Horace Pinching, who had been so sympathetic towards this work, retired in the autumn of 1907 from his post of Director-General of the Department. His successor immediately told me that he did not consider it necessary for me to continue the work, and he treated me in such a manner that I was obliged to resign my appointment in the Department. At the same time my brother, E. H. Ross, was also threatened in my presence with dismissal. It will therefore be understood that since these events the Government has become lethargic about anti-mosquito measures. I have heard that mosquitos have returned in the parts campaigned

in Cairo; and at Heluan, and even Port Said, the pest is reappearing. I have seen frequent complaints in the Egyptain newspapers that the campaigns have been allowed to fail, and I fear that for the time being mosquito destruction has fallen into disrepute in Egypt.

From my experience of the Public Health Department, I think that the Government does not appear to appreciate its responsibility regarding sanitary administration. A visit to Cairo will show that nearly all improvements are made by private enterprise. When a new town is started, such as Heliopolis Oasis, it is started by a company. The most modern buildings in the city itself, erected for the public, have been built by His Highness, the Khedive, on his own private property. Yet the Government owns nearly all the vacant land round the city, and it refuses to part with it. The result is that the city is becoming overcrowded, with an enormous death-rate. Calculated on the 1907 census, the death-rate of Cairo was, for 1908, 39·23 per 1,000, and for 1909, 44·9 per 1,000. In an article published in the *British Medical Journal* on 10th April 1910, which describes the state of the sanitation of Cairo, it is stated that 28·2% of the infants born in Cairo die during the first year of life; and 48·5%, or nearly half, of the children born will, if the present death-rate continues, die before they reach the age of sixteen years. When I was in Cairo, in certain districts in the Saida Zenab quarter, known as El Mardi and El Sakkia, Government properties, the death-rate reached 20%, and yet the Government will not allow the city to expand, and appears to await a rise in the value of the property before it will part with it. My personal opinion is that the fault is due to the fact that the country is governed almost by amateurs. The sanitary officers are not bound to obtain Public Health Diplomas, and it seems to me that many of the higher Government offices are placed in the hands of friends and relatives of existing officials, apparently without regard as to qualification. I have shown that the laws are difficult to enforce. There is no building

law whatever; anybody can build any hovel he pleases. Few reports are published from the Public Health Department, and officials under the present *régime* are not permitted to print the results of their work for the benefit of their successors.

Mosquito extermination, however, is bound to be undertaken seriously in Egypt under more competent management. For the time being, perhaps, it will remain in abeyance, but it is a measure which is so certain in its results and so far-reaching in the good which it does, that before long, I think, it will be generally demanded by the public. It is nauseating to live in Cairo in the summer and to be bitten persistently by insects which breed in excrement. Mosquito destruction is an unpleasant though a simple problem. It must be continued unremittingly, or the gnats will return; but the cost of the campaign will fall to a certain extent after the initial destruction of the pest. I hope that the work will be started throughout Cairo, for I know that the results will be greater than were ever anticipated.

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54. The Campaign at Khartoum. — In the pre-Dervish days Khartoum must have been a hot-bed of mosquito life. This is apparent from the evidence of Sir Rudolph Baron von Slatin, who states that mosquitos were very prevalent during this period, and from that of Father Ohrwalder of the Austrian Mission, who asserts that he was sometimes driven to go and stand up to his neck in the Nile in order to avoid the attentions of the winged hordes. That malaria was also present cannot be doubted. Schweinfurth speaks of the unhealthiness of the place in a manner which leaves little doubt but that one of the chief causes of the high mortality which obtained at certain periods was malarial fever. It is believed that its evil reputation was one of the reasons which induced the Mahdi to abandon its site altogether, and, with that empirical wisdom which often characterises the native, to found Omdurman on a spot where the river banks are not bold and terraced but shelving, and consequently free from pools at low Nile. Some four years after the new city had come into being, a survey of the conditions as regards mosquito prevalence was made. This disclosed the fact that three genera, represented by three species, were constantly present, namely, in order of frequency, *Culex fatigans*, *Stegomyia calopus* and *Pyretophorus costalis*. Malaria was found to exist, and it was shown that infection could be acquired locally, while numerous cases were always being brought into the town by steamers coming from the highly malarious regions to the south. It was towards the close of

1903 that anti-mosquito measures were instituted and a small mosquito brigade established. These measures have continued in force ever since, and the brigade has been constantly employed; for one of the chief points elucidated by the work is the absolute necessity for its continuity. In a place like Khartoum it is essential to carry out preventive measures year in and year out, and two mottoes most suitable for those in charge might well be "Weary not in well-doing," and "Do not put overmuch trust in the native inspector."

In this brief note we are only concerned with the single species of Anopheline which constitutes itself a danger to the town, and may omit any detailed account of the work in connection with the two other species of Culicid mentioned; but it is well to point out that in practice these latter cannot be neglected. Mosquito prevention measures must be general, and it is very often the operations against *Culex* or *Stegomyia* which lead to the discovery of invasion or threatened invasion by *Pyretophorus*.

P. costalis, then, is the malaria carrier of the Northern Sudan, and so far as Khartoum is concerned its breeding-places were found to consist of:—

(1) Pools left by the falling Blue Nile. These are found for the most part in the sand-banks which appear in the river's bed, but may also occur in the sloping and terraced banks as the river falls.

(2) Garden tanks.

(3) Bath-waste pits.

(4) Permanent garden pools.

(5) Sakia pits, as used in irrigation work.

(6) Irrigation channels and pools formed by leaking "gudwals," as the large and small canals are termed. The only large area of irrigated land is at Khartoum North.

(7) Wells.

(8) Rain-water collections.

(9) Household water collections, such as water stored in barrels. These are not at all common.

(10) Water collections on steamers, provided these be exposed to the light, *i.e.*, in boiler trays, cisterns, etc.

(11) Chance water collections, as in old barges and leaky boats where speedy water evaporation is prevented.

One or two of these breeding-places merit a few words of explanation. The soil in some parts is what is termed "weeping." It contains a large quantity of salt. Hence if gudwals are constructed of this soil the water dissolves out the salts, and leakages soon result, pools form, and, despite the fact that the water is brackish, *P. costalis* readily lays her eggs in such collections. This mosquito is very rarely found in wells, but its larvae have once been encountered in a well 70 feet in depth. This is so peculiar that one wonders if they had not been accidentally introduced. Rain-water collections are rare in Khartoum, but occasionally the summer is comparatively wet, and then pools and puddles may persist for some time. They are not commonly utilised as breeding-places by *P. costalis.* This Anopheline is not often found breeding out on the river steamers, but has been known to do so, and may be introduced into Khartoum as *imagines* both by steamers and by native boats. Recently a species of *Nyssorhynchus* has been apparently introduced in this manner. It is worth noting that since operations were started cases of malaria locally acquired in Khartoum have never once occurred without the presence of Anophelines in or near the town being demonstrated.

The chief methods of prevention and reduction put in force may be tabulated as follows:—

(1) Systematic inspection.

(2) Notification of cases of malaria.

(3) The issue of warning notices and distribution of information.

(4) Petrolage.

(5) Filling in of pools and abolition of water collections such as garden tanks, bath-waste pits, etc. Also the screening of cisterns.

(6) The introduction of fish into water channels.

(7) The piping and covering of raised water channels to prevent leakage through their banks.

(8) The imposition of fines.

(9) General legislation as regards borrow pits, irrigation, engineering work, and the provision and maintenance of dry zones.

All three forms of malaria used to occur in Khartoum. Benign tertian was the most common, malignant next, while quartan was rather infrequently encountered, though it has been found to be much commoner than was at first supposed. Quite a large number of cases harbouring crescents annually come into the town from the Upper Nile and the Bahr-el-Ghazal Province.

Some of these measures call for a little further notice. The inspection must be thorough and the native inspector must be controlled by trained and reliable British sanitary inspectors. The town is divided into sections, each of which is in charge of a native who has no sooner completed his round than he begins again. In this way, if he does his work properly the inspector should visit every house and every water collection in his district at least once a week. The British inspectors are responsible for the whole town, and often combine mosquito work with house-to-house inspection. Areas bordering on the river, the point of danger, are in charge of the most experienced men, and a special native inspector is detailed for steamer and boat work. Khartoum has a population of some 16,000; Khartoum North, of about 25,000; and there are in all at the present time three British sanitary inspectors[1] and seven native inspectors. Of these one British and two native inspectors are responsible for Khartoum North, where with

[1] Mosquito prevention is, of course, only a part, though an important one, of the work of these inspectors.

the exception of some pools on a sandy island there are no river pools, the banks being high and steep, and, where there are scarcely any gardens, the only dangerous area, so far as *P. costalis* is concerned, being a large stretch of irrigated land and certain small irrigation channels which are apt to leak and form pools. The dockyard is situated at Khartoum North, and the steamers require constant attention, but more for *Culex* and *Stegomyia* than for *Pyretophorus*. When necessary, extra men are employed filling in river pools; and if these happen, as at present, to exist in sand-banks opposite the so-called British Barracks, the services of the British troops tenanting these latter are at once solicited, and never in vain.

Notification has proved of great value. It stimulates enquiry and has often led to the discovery of breeding-places when such would otherwise have been missed.

Here are types of the warning notices inserted at intervals in the local press, especially during the winter when the Nile is falling, and again during the brief rainy season in August and September.

SANITARY NOTICES

"The attention of householders and others is again called to the necessity of properly emptying and drying out zeers and other vessels holding water at least once a week, in order to prevent mosquitos finding breeding-places. Persons proceeding on leave are especially requested to see that this is done before premises are left vacant. Any one found harbouring mosquito larvae or pupae on his premises is liable to be punished in accordance with Town Regulations; and in future stringent action will be taken to put an end to this nuisance and danger to health. It is not enough to instruct servants to carry out these measures. There must be personal supervision. Five minutes per week is no great tax on the time of any one."

"Householders are urgently requested to give immediate notice to the Sanitary Inspector at the Mudiria if they find themselves troubled by mosquitos or note their presence. Any one found harbouring larvae or pupae on his premises is liable to be fined; and stringent action will be taken, as malaria-carrying species are at present endeavouring to obtain a footing in the town."

The regulations as regards steamers are of practical interest and are also given below. When carefully followed the results have been excellent.

"1. Before leaving Khartoum the bilge water in the various sections should be oiled by pouring petroleum on the surface of the water and stirring well with a stick. A film will then form on the surface, which prevents access of air to the mosquito larvae and pupae, and so kills them. Eggs deposited by mosquitos on this oil film will not develop, and many of the females themselves will be killed. Roughly, about one half-pint of oil should be devoted to each section. A little experience soon shows how much or how little oil is needed to form a proper film. In addition, the water in the trays under the boilers should be examined, and, if necessary, oiled.

"A cup or wide-mouthed bottle can be used for collecting water for examination. The vessel should be quickly but gently dipped under the surface and lifted out without spilling any of the contents. For somewhat inaccessible places a tin with its bottom replaced by wire-gauze mesh and attached to a stick is useful. The larvae are then found wriggling on the gauze. Such a collecting dish is easily made and answers well. It should be examined in a good light. It is to be remembered that any standing water collections will harbour mosquito larvae, and attention should therefore be paid to the water-closet cisterns, zeers and tanks.

"2. During the voyage, opportunity should be taken to re-oil the bilges wherever possible. This should especially be the case before starting on the return journey. Wood holds are usually at fault. The difficulty can be got over by emptying these in rotation, so that the water in the hold can be got at and oiled once in every fortnight. Special attention should be directed to zeers when these are used for storing bottles of drink. All zeers should be emptied out at least once a week during the voyage.

"3. The thing to be avoided is returning to Khartoum with mosquitos on board. If this is done, wells and water collections in the town which have been cleared at trouble and expense are liable to become reinfected. This has happened repeatedly, so that it is very important that steamers should arrive clean and free. It is well to have all bilge water emptied on arrival, and all steamers lying up should be inspected and treated in the manner described. Similar precautions are required as regards barges, sandals, launches and any vessel on which there is stagnant water.

"*Note.*—It has been proved that mosquitos, as a rule, will not stay for any length of time on a steamer if they are prevented from breeding out on board. Consequently, such preventive methods are effective, as has been demonstrated on several occasions; and there is no excuse, in most instances, for steamers reaching Khartoum with their bilge water, etc., full of larvae and pupae, and their cabins full of adult mosquitos.

"Sometimes it has been found advisable to employ sulphur squibs when there were many adult insects in the holds."

A little space may be devoted to the introduction of fish which has been successfully employed in the irrigation channels at Khartoum North and, as will be seen, elsewhere in the Sudan.

A zealous and interested official some time ago suggested the introduction of the small "millions" fish (*Girardinus poecilloides*) from Barbados to cope with larvae in swamps, irrigation channels, large pools and like collections of water. The question was referred to me, and I found myself opposed to the idea, partly because I know the difficulty of transporting these fish, and the possibility that they might not take kindly to new surroundings, and partly because, like the proud Syrian of old, I was tempted to reply: "Are not Abana and Pharpar, rivers of Damascus, better than all the waters of Israel?" In other words, one was of opinion that there was no use going outside for what might very well be obtained at home. I knew that Captain Flower, Director of the Zoological Gardens at Ghizeh, employed young "Bulti" (*Tilapia nilotica*) in the tanks of the aquarium at Ghezireh, and found them most efficient in keeping the water free from mosquito larvae, and it was reasonable to suppose that many Nile fish were effective, especially as it is found that when fish are present in any pool left by the falling river, mosquitos do not breed out therein.

There were two points, however, requiring attention. If any water collection is stocked with fish, these latter are apt to be speedily cleared out by natives or birds, such as king-fishers, which are very plentiful. Moreover, as is explained later, fish

which attain any size are not suitable for use on irrigated land. Hence, it was desirable to find a small and greedy fish which would offer no temptation to the native, which could be used in irrigation channels, and which, though not immune from the depredations of fish-loving birds, yet was so prolific, or could be used in such numbers, as to make these ravages of little account. The type of fish desired was that known in India as the Chilwa (*Chula argentea*), which is said to be more efficient than "millions," as not only does it devour the larvae but, being a surface-feeder and an eager fly-taker, it is deadly to the adult mosquito, especially to the female intent on egg-laying.

As Mr Harold King, our entomologist, was going up the White Nile in the *Floating Laboratory* on a voyage of discovery, I asked him to take every opportunity of studying the question. Mr King is an excellent field-naturalist, and admirably fitted to carry out such an investigation. His report, which I append, will be found of interest and value, and it would appear that in *Cyprinodon dispar* we have obtained the fish we require. This, as Mr King remarks, remains to be proved, but the following letter, kindly sent me by the Acting Manager, Sudan Plantations Syndicate, in whose irrigation channels the fish are being tested, is at least promising:—

"DEAR SIR,—In reply to your favour of the 19th inst. I have examined the small canal into which the fish have been introduced.

"The canal is about thirty metres long and about half a metre deep, and the opening to the larger canal is closed with gauze-wire netting to keep other fish out. Although the still water is now lying twenty-five days there are no signs of mosquito larvae.

"The fish are alive and seem to feed on flies on the surface of the water, and are most active in the early morning.—Yours faithfully, ALEX. MACINTYRE."

Like Mr King, I have fed *Ophiocephalus obscurus* (*vide infra*) on the larvae of *Pyretophorus costalis*, and noted how the

latter were devoured before ever they reached the bottom of the jar.

The following is Mr King's report:—

"While on the White Nile, between the dates of 17th April and 19th July, I carried out some investigations with Nile fish with a view to ascertaining what species, if any, were likely to be of value in controlling mosquitos by feeding on their larvae. In most of the swamps, where mosquito larvae abounded, shoals of small fish of various species also occurred, but from the fact that the two throve together it was evident that the fish were not likely to be of very much use. Numbers of these fish were captured, placed in jars and offered mosquito larvae, but with one exception they refused them until they had passed one or more days without food. The exception was a fish found in a khor between Gebel Ahmed Aga and the river. In this khor very few mosquito larvae could be found—a search of nearly two hours resulted in less than a dozen specimens being taken—but there were present numbers of the young fry which has been identified by Mr G. Boulenger of the British Museum as *Ophiocephalus obscurus*.

"This fish feeds greedily on mosquito larvae, thrives well in captivity and in stagnant water, and appears to be an ideal fish for the purpose for which it is wanted, except in one respect, viz., size. When full grown it attains a length of 35 centimetres. As fry, inhabiting shallow waters such as are found in swamps and river-spills, this fish would no doubt prove of value, for it is in just such places that mosquitos breed. On the other hand, though the fish would flourish in large irrigation canals, and the fry could reach the smaller gudwals on irrigated land, the latter are frequently so constructed that the young fish on increasing in size could not regain the parent canals and would perish. Hence this species of fish is not likely to be an economical means of dealing with mosquito larvae in such situations.

"Some thirty specimens of *O. obscurus* are still living in a jar in the Laboratories.

"During May, I met Mr Butler, superintendent of game preservation, who told me of a small fish—*Cyprinodon dispar*—which lives in Khor Arbat, about twenty-two miles from Port Sudan. He very kindly furnished me with some notes on its habits, and recommended that it should be given a trial as it appeared to him to be likely to give good results. He further said that, while no mosquitos are noticeable in the vicinity of

that khor, they are plentiful near other similar khors where *C. dispar* does not exist.

"Accordingly, on 6th September, I left for Khor Arbat and, having spent three days there, returned to Khartoum on 12th September. There were myriads of the fish in the khor, but at first I had considerable difficulty in persuading them to live in captivity. Eventually, I found that while, if placed in jars nearly full of water, most of them died within twelve hours, yet if given only about two inches of water over a layer of sand they could be transported fairly easily. One hundred and one living specimens reached Khartoum, and were there offered mosquito larvae, which they took readily. This, however, is not conclusive proof that *C. dispar* feeds on mosquito larvae in its wild state, as the specimens had been unfed for two days when the larvae were given them, none being obtainable at Khor Arbat.

"As these fish did not thrive in jars I wrote to Mr MacIntyre, Acting Manager of the estate at Zeidab, belonging to the Sudan Plantations Syndicate Ltd., asking him if he would allow them to be placed in a gudwal on the Company's estate for purposes of experiment. This he readily agreed to; so that on 19th September I took those that were still alive—about seventy in number—to Zeidab, and the following morning liberated them in a short length of gudwal which Mr MacIntyre very kindly had filled with water. This gudwal is not used, at present, for agricultural purposes, and the pipe connecting it with the canal has been netted to prevent other fish gaining access. This was done in order that the *C. dispar* might be given every chance of establishing themselves before being subjected to the possible ravages of other and larger fish.

"The specimens liberated at Zeidab were all immature, as it was found that partially-grown fish withstood captivity better than did adults.

"When full grown, *C. dispar* attains a length of 8 centimetres, and in all stages appears to prefer shallow water.

"Should the gudwal in which these fish were liberated remain free from mosquito larvae during the coming year, while similar gudwals in the immediate vicinity serve as breeding-places for these pests, I think sufficient proof of their value will have been obtained to justify an effort being made on a larger scale to establish *C. dispar* throughout all the gudwals on the Sudan Plantations Syndicate Ltd.'s estate, and on other similar farms where great difficulty is experienced in controlling mosquitos."

As regards legislation the following irrigation rules, imposed

on all who take up land with a view to cultivation on a large scale, may be quoted with advantage. They are of very great importance both from the sanitary and the economic standpoint.

"1. Irrigation channels should be constructed on a higher level than the surrounding land, so that when the flow of water in them ceases they may drain dry.

"2. They should be constructed of such material and in such a manner as to prevent leakage.

"3. Their banks and beds should be kept in good repair, and the beds even, to prevent the formation of pools.

"4. 'Dead ends' of irrigation channels should be reduced to the smallest size compatible with efficiency, so that water will not stagnate in them.

"5. Vegetation should be periodically cleared out of the channels.

"6. Sluices should be constructed so that there is no leakage to form stagnant puddles.

"7. Where possible, fish should be introduced, and kept in the main channels to destroy the larvae.

"8. Lands where water is apt to stand should have proper surface drainage.

"9. Crops, such as sugar-cane, rice and others which require to stand in water, should not be grown within half a mile of any town or village.

"10. If an engine or pump should happen to break down, particular care should be taken to deal with stagnant pools, and petroleum should be used when necessary.

"11. Cases of malarial fever, and any prevalence of mosquitos, should be notified to the governor of the provinces by the manager of the concession."

The above is a brief outline of the measures adopted and the work accomplished. What has been the result? It is difficult to give comparative statistics, as we do not know how much malaria used to be locally acquired in Khartoum; but we do know that *P. costalis* used to be fairly common, especially at certain times of the year; while now it is wholly absent during many months, and if it does invade the town its breeding operations are usually quickly checked, it does not multiply, and soon disappears. As a direct consequence locally acquired malaria has become very rare. One cannot

wholly trust the notifications, but they afford considerable indication of how matters stand, and, so far as Egyptian and British troops are concerned, are trustworthy. Here, then, are the locally acquired cases for the past few years.

Year Oct. to Sept. inclusive.	Civil population.	Egyptian military About 3,000 men most of them very susceptible to infection.	British troops (One battalion) about 600 men changed annually.
1904-05	Seven cases in all. Not stated according to population attacked. Unsatisfactory notification.		
1905-06	2 (?)	?	8
1906-07	4	4	1
1907-08	2	1	0
1908-09	1	4	2
1909-10 (to date) (16th January 1910)	2	...	4

It is very rare for a British official or European resident to acquire malaria in Khartoum itself, while I should say that owing to increased trade and communication more cases are now imported than was previously the case. There can be no doubt that if operations were suspended Khartoum would speedily regain, if not its old evil notoriety, at least a certain measure of the same. At the time of writing there is a good deal of malaria in the neighbourhood of the town, *i.e.*, in villages a couple of miles to the north, in one part of Omdurman, and at various places along the White Nile. Anophelines now, as previously, are endeavouring to obtain a footing in the town, and have been blown into it by the wind or brought into it by boats. As a result, we have had a few cases, mostly in British soldiers who were wont to wander out to the east of their quarters towards a part of the river where there are extensive sand-banks and many pools, some $2\frac{1}{2}$ miles from the centre of the city. Had operations not been in force I am very certain that this year would have witnessed an epidemic; for conditions seem specially favourable to the propagation of *P. costalis* at present, and, as stated, there is much malaria

in the neighbourhood. A measure which has had good effects is to put any dangerous area out of bounds for British troops.

One may conclude by stating that Khartoum is a favourable place for carrying Professor Ross's methods into effect; and that they have met with marked success, and doubtless will continue to do so, provided the work is carried out continually, thoroughly, consistently and with intelligence, that the influence of immigration is borne in mind, and that legislation provides for the control of irrigation and the regulation of irrigated areas.

By Dr L. BOSTOCK

District Surgeon, Komatipoort, Transvaal

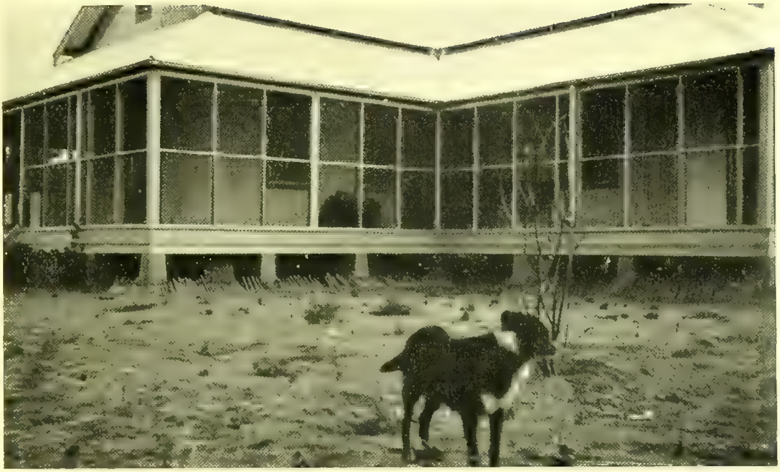
55. Malaria in South Africa.—British South Africa as a whole does not suffer much from malaria. Along the east coast, in Portuguese East Africa, Swaziland and Zululand, the disease is common enough, and occasional outbreaks occur in a few parts of the Cape Colony and Natal. A recent outbreak in Durban assumed serious proportions.

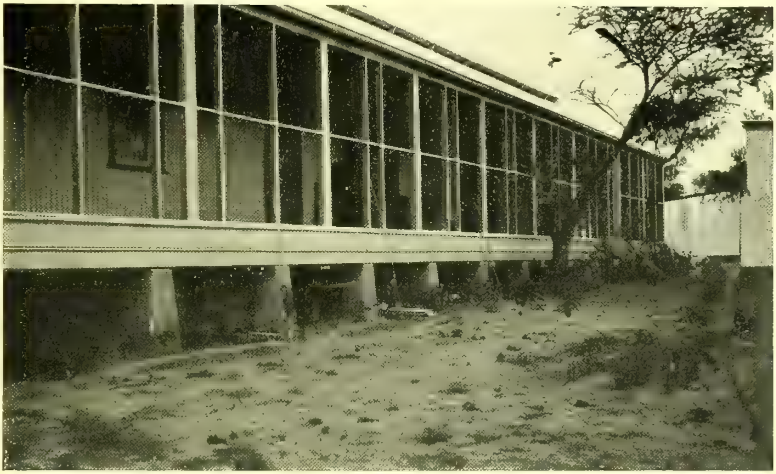
In the Transvaal the disease is endemic in the Low Veldt, in the east and north of the country; and the type is severe, bilious, remittent and haemoglobinuric forms being common. Owing to paucity of population in these districts the disease attracted little attention until the building of the Pretoria-Delagoa Bay railway line, when the heavy death-rate forced the disease upon the public notice. No effort was, however, made to combat the disease until I was appointed District Surgeon here, and commenced to make representations to Government on the subject in 1903. The work was started in 1904, and immediately its results were obvious in a greatly reduced sick-rate. The Railways thereupon instructed me to make a fever survey of the main line and the Barberton branch, and to make recommendations for each station. I obtained the assistance of the Government Entomologist, the late Mr C. B. Simpson, for this survey, and our recommendations, being promptly carried out, resulted in a considerable reduction of fever along the line.

Every station and ganger's cottage from Komatipoort to Waterval Onder was dealt with, local measures for mosquito reduction carried out, and all railway quarters in the Low Veldt made mosquito-proof. We were compelled to rely chiefly on the latter method of fever prevention, owing to the small number of persons concerned, and the great amount of work necessary to deal with the breeding-places along the Crocodile River, which runs parallel to the line. All small breeding-places in the immediate neighbourhood of the stations were dealt with.

To the Central South African Railway belongs the credit of first efforts to combat malaria in the Transvaal, and since 1905 the apathy of the public and the authorities has been gradually overcome. The results obtained by the Central South African Railway proved a powerful argument, and now the value of anti-malarial measures is fully recognised by private residents and the different Government departments who have officials in this part of the Transvaal. Practically every private resident in this town has now a mosquito-proof bedroom; and in the country many farmers have also adopted this precaution. Mosquito-proof quarters are being provided for all police posts in the Low Veldt, and also for the officials of the Government Game Reserve, which lies north of the Crocodile River. The Colonial Secretary's Department have now arranged to issue free quinine in all fever districts in the Transvaal to all indigent whites and all natives; the Barberton municipality have lately taken up the subject; and a League has been founded for the purpose of carrying on the anti-malarial campaign throughout the district.

Anti-malaria methods employed at Komatipoort. — Anti-malarial work was commenced at Komatipoort in 1904, the cost being borne by the Central South African Railways. The town is rather unfortunately situated, in the angle between the Crocodile and the Komati Rivers. Along both rivers are





I. AN EXCELLENT TYPE OF MOSQUITO-PROOF HOUSE AS BUILT BY THE TRANSVAAL GOVERNMENT.

The residence of Major Hamilton, Warden of the Sabi Government Game Reserve. Views of front and back of house.

A SHOOTING CAMP ON THE LOW VELDT.
Portable folding mosquito-proof beds.

2. AN EXAMPLE OF ROUGH BUT EFFICIENT MOSQUITO PROOFING IN THE VELDT.
A hut at Sabi Bridge, Game Reserve, Transvaal, formerly occupied by the Warden, Major Hamilton.

AN AFRICAN "RONDHAVEL," AS MOSQUITO-PROOF SLEEPING-QUARTERS.

3. AN INGENIOUS IDEA.

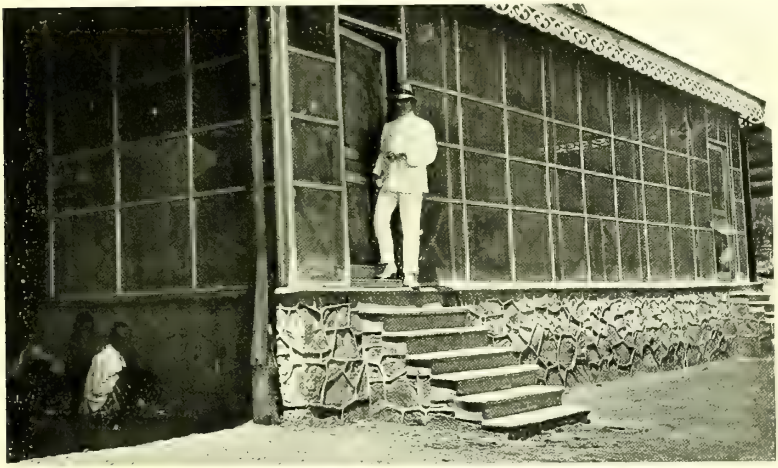
A military block house, bought as it stood after the war, converted by addition of door and windows into a very comfortable mosquito-proof bedroom.

TWO-ROOMED WOOD AND IRON HOUSE WITH MOSQUITO-PROOF VERANDA.

As built by the C.S.A.R. for temporary quarters at the Sabi Bridge, Selati Railway. Cost about £180.

4. A GOOD EXAMPLE OF MOSQUITO-PROOF HOUSE.

The Customs quarters, Komatipoort.



5. JOINT OFFICES OF THE WITWATERSRAND NATIVE LABOUR ASSOCIATION, AND THE PORTUGUESE DEPARTMENT OF EMIGRATION AT RESSANE GARCIA, PORTUGUESE EAST AFRICA.

Natives receiving their passes through the sliding window in the netting. (The tear is in the photo film, not in the netting.)

6. ACTUAL BREEDING-PLACES OF ANOPHELINE MOSQUITOS ON THE CROCODILE RIVER, EASTERN TRANSVAAL.

Typical of many places along the river, where the broken rocky ground holds many small pools after rain, or when the river is falling.

7. COMFORT WITH SAFETY.

A mosquito-proof veranda as a bedroom in the hot weather (used by the author).

8. ACTUAL BREEDING-PLACES OF ANOPHELINE MOSQUITOS IN THE EASTERN TRANSVAAL.
Taken near Nelspruit Station, C.S.A.R.

backwaters, pools and inlets, with dense vegetation in parts, and below the junction lie some acres of broken, rocky ground holding innumerable small pools. The worst breeding-place of Anophelines, however, was a swamp running parallel to the railway and opposite the station. The swamp was fed by a small spring and surface drainage, and by the waste water from the pump and engine sheds. It consisted of about two acres of ground only, but the whole of it was a network of small pools amongst thick reeds and rank vegetation. Anopheline mosquitos were breeding here in great abundance, and it was decided to obliterate this swamp entirely. A brick and cement drain was cut through the centre to carry off the water supply which maintained it. The ground was cleared, all pools filled in, and the surface evenly graded from each side down to the drain to carry off the rainfall. The reclaimed ground was planted with banana trees and eucalyptus, and absorption of water by these has sufficed to keep the area perfectly dry, and the former swamp is now, on a small scale, a town plantation. Efficient treatment of the main rivers was impossible on the score of expense; but the portions of them within half a mile of the town were dealt with by filling in some pools by rubble and cement, and, a mosquito gang having been organised, larger pools were treated with paraffin.

These methods proved in practice to be most satisfactory. Only partial reduction of mosquitos had been attempted, but it was subsequently found most difficult to discover any specimens of Anophelines within the town area.

At the same time all railway servants' quarters were made mosquito-proof, and the reduction of fever cases was marked and immediate.

The following table is taken from a report on the subject made to the General Manager of the Central South African Railway in December 1905.

Cases of malarial fever among Central South African Railway employees—Komatipoort—Kaapmuiden section:—

	1903	1904	1905
January	212	396	55
February	230	341	42
March	277	405	43
April	281	320	33
May	207	192	40
June	173	60	20
July	175	61	10
August	209	63	4
September	80	38	18
October	191	41	28
November	234	20	9
December	217	25	8 (to 7th Dec.)

The above figures are gross details, and represent each case counted for every day of illness, that is, they are "days lost" by the whole of the railway staff. The actual daily average of the number of sick during March 1905 is 1·38, against a similar average for the same month in 1904 of 13·06.

The work of proofing the quarters was begun in June 1904.

Portuguese East Africa.—With the exception of the town and district of Lourenço Marques nothing has been undertaken, but the Portuguese authorities have taken up the subject with great energy in the town of Lourenço Marques, with the result that the town, once extremely unhealthy, is now comparatively free of the disease.

The Municipal Health Officer, Dr Amaral Leal, has controlled the work, while the Government Entomologist, Mr C. W. Howard, has carried out an investigation of the local mosquito problem. Their methods have been the draining of a swamp, which was the chief breeding-ground; organisation of anti-malarial sanitary measures; appointment of special sanitary inspectors, with power to inspect all houses and compounds, and to enforce the carrying out of their instructions; the treatment of all breeding-places; and the netting of Government offices and quarters.[1]

[1] In speaking of anti-malarial measures in Lourenço Marques, I should like to put on record the great service rendered on this subject by A. W. Bayly, Esq., the

BY P. MURISON, M.D., B.Sc., D.P.H.
Medical Officer of Health, Durban

56. **Epidemic of Malaria in Durban, 1905-1907.** — Malaria broke out in Durban during January 1905, and the number of cases rapidly increased until the cooler autumn weather set in. The following table shows the monthly notifications:—

Summer.		Autumn.			Winter.	
January.	February.	March.	April.	May.	June.	July.
12	85	612	1,084	1,877	497	135

Some parts of Zululand are so unhealthy from the presence of malaria that they are uninhabitable.

In Durban, malaria has been a compulsorily notifiable infectious disease since 1902, and as a result an average of seventy cases were notified per annum, as existing in the borough. All these cases were found on enquiry to have contracted the disease elsewhere, and had come to Durban to recuperate.

No definite information, either from professional or lay persons, can be obtained to show that malaria had ever existed as a local infection within the borough of Durban previous to 1905. Within a fortnight of the 1905 outbreak every medical man in Durban had recognised many cases.

The borough of Durban consists somewhat roughly of a triangle, one side being bounded by the Indian Ocean, another by the waters of Durban Bay, and the base is formed by a range of low hills, in extent about 6 miles, which stretches from one end of the borough to the other. From the base of these hills to the water's edge there is a plain, being about

$1\frac{1}{4}$ miles in greatest width, and probably averaging about 15 feet above high-water mark. All classes of houses are to be found on this plain.

Chiefly along the base of these hills, this flat area is at its lowest level, and contains many swampy areas, some of quite small size, others extending to scores of acres.

The slope of the hills (Berea) extend to, roughly, about a mile from the plain to their summit, and on this gradual rising ground have been erected dwellings of all classes.

The notifications received during the first few months distinctly localised malaria into many areas, and these were found to be always in close proximity to swampy or water-logged lands.

When malaria broke out, I decided to rigidly follow the teachings of Professor Ronald Ross, and this has been adhered to throughout.

It was recognised from the beginning that in order to cope with the disease both temporary and permanent measures were necessary.

The *permanent improvements* were to consist of such work as (1) The drainage of surface and water-logged and swampy areas. The cutting of new watercourses and the extension of old ones. (2) The filling in of swampy areas, pools, etc., and the levelling and grading of land to enable stormwater to flow towards some drain or watercourse. Such measures naturally removed for all time the pools and swamps necessary for the larvae stage of mosquito development. Since 1905 the borough has expended on this class of work approximately £80,000.

The *temporary measures* consisted of the formation of a mosquito brigade, whose work was to consist of dealing immediately with all pools or areas of swampy lands which were the habitat of mosquito larvae by means of larvicides such as paraffin and disinfectants.

We discovered that the crudest and cheapest disinfectants

were the best for larvae destruction, and we used disinfectants where pools were shallow, or where reeds existed in considerable numbers. We found it not only cheaper but more efficient to use disinfectant under such circumstances. One part of disinfectant to five thousand parts of estimated water in the pool was found to be effectual. Paraffin was used where pools were deep, and where an unbroken film could be formed.

Many substances were tried as larvicides, and some mechanical ingenuity was shown by those in charge of the work in endeavouring to improve the process of application of the larvicides.

As permanent improvements are carried out, the area of temporary measures was naturally reduced, and it may be stated that the only part of the borough of Durban requiring any temporary measures now is practically confined to that part known as the Eastern Vlei.

It was also recognised that the education of the inhabitants of the borough regarding the natural history of malaria was a necessary auxiliary aid in the work of prevention. We believed that a much more whole-hearted assistance would be more readily obtained if some facts regarding the disease were freely spread about so that the rationale of the work and the orders required to be carried out by householders were more clearly understood and appreciated. Hand-bills dealing with malaria prevention were left at all houses in which cases of malaria had been notified to exist, and in some areas these hand-bills were left at every house. Lectures and magic lantern demonstrations were given by me to societies, institutions, schools, etc.; and I feel sure these lectures and demonstrations were a potent agent for good in our campaign.

The mosquito brigade consisted of two Europeans and about fifty Indians. Every swampy part where larvae could breed out being treated every ten days or thereby, so as to render their development impossible.

Present position.—In 1907 the number of freshly-infected

cases were very few in number. From a circular issued to all the medical practitioners of Durban (for we had stopped compulsory notification of the disease owing to the excessive cost in 1906), the total number of cases would probably be less than a hundred. At the same time, immediately outside the borough, every household, European and coloured, was down with the disease, and the deaths amongst Indians in particular were enormous. This, in my opinion, was a brilliant object lesson as to the efficiency of the preventive measures employed within the borough. Durban can be invaded from the neighbouring country from both ends, the swampy lands here being continuous. On the reverse slope from Durban of the Berea hills the descent to the Umgeni Valley is fairly steep. This valley was saturated with malaria in 1907, but no invasion of the borough took place from this direction. The hill (500 feet) seems to act as an effectual barrier. At each end of the ridge, however, where invasion did occur, an area or zone was formed, in which permanent and temporary measures were carried out as efficiently and quickly as possible. Fresh cases which occurred in the borough in that year were almost wholly confined to these areas. Government recognised the necessity for dealing with the excessive sickness and mortality in the area lying in contiguity to our borough, and started an anti-malarial movement towards the end of the season, but too late to be of any practical benefit for that year. In Durban during 1908 permanent improvements were still carried out and temporary measures maintained. No cases occurred within the borough. Outside the borough, where the disease had been rampant the previous year, and where no preventive measures of any kind were being taken, no fresh cases of malaria occurred. The disease seemed to have suddenly left Durban County.

The measures which have been carried out in Durban in order to exterminate malaria have added very much to the comfort of life, as well as to the salubrity of the town. Water

tanks and cisterns which were an adjunct of every house, although rendered quite unnecessary owing to the bountiful supply of water introduced into Durban, were still retained in position full of water, probably more through apathy than anything else. These were all ordered to be removed by the Sanitary Department. These tanks did not form breeding-places for *Anopheles*, but *Culex* developed in them in great numbers. With the vigilance which is still being maintained, Durban may be said to be at present practically free from mosquitos of any kind. In fact, the presence of a few mosquitos in a house is at once communicated to the Sanitary Department in order that the nuisance may be abated. Contrasted with the existing state of matters five years ago, I can only describe it as marvellous, and no people ought to be more grateful to Professor Ross of Liverpool than the inhabitants of Durban, both for the practical benefits resulting from his brilliant researches into the etiology of malaria, and for the effectual preventive measures he so simply enunciated, and which have been demonstrated and proved in this borough.

By MALCOLM WATSON, M.D., D.P.H.
Chief Medical Officer Estate Hospitals Association
Late Senior District Surgeon, Federated Malay States Medical Service

Abstract by R. Ross

57. The Prevention of Malaria in the Federated Malay States.—The contribution to this book sent by Dr Watson contains a full and vivid description of his enthusiastic, large and successful campaign, carried out during the last nine years. Unfortunately, it is too long for insertion here ; but owing to the interest of Sir Frank Swettenham, G.C.M.G., and a number of Rubber Companies, it will shortly be published as a separate book. I will here attempt to give only a brief abstract.

The campaign was conducted chiefly in the State of Selangor. The climate is warm and humid; the mean rainfall is 80·4 inches; and the country was originally covered with thick jungle, and belonged to a number of independent states. Owing, however, to the efforts of Sir Frank Swettenham, these states were amalgamated into a Federation, since when there has been much progress in the country. By reason of its products and its good Government, it is now a comparatively wealthy and prosperous one. The history of malaria prevention in it dates from 1901, and is concerned (*a*) with prevention in the towns, and (*b*) with prevention in the district.

(1). *Towns.*—The two principal towns are Klang and Port Swettenham. In 1901, Klang, situated on the river of that name, contained 3,576 inhabitants, occupying 293 houses. The area of the town was about 290 acres, of which 22 acres were swamps, 25 acres virgin jungle, and 80 acres dense secondary growth, 30 to 40 feet high. Owing to the humidity mosquitos

bred in vast numbers all the year round. During that year the author says that the outbreak of malaria reached terrible proportions. Nearly every one was attacked, and many died. The whole population was demoralised, and when, in November, the death-rate rose to the equivalent of an annual death-rate of 300 per mille, the Chinese suspended business. In the meantime, however, Watson resolved to make a determined stand against the disease, and after consideration decided to adopt my proposals for mosquito reduction. In this he was warmly supported by Dr E. A. O. Travers, State Surgeon, by the Government, the Sanitary Board, and the people. The requisite money was voted; the marshes were rapidly drained, chiefly by contour drains which cut off the inflow. The author says, "I must confess that I by no means expected the success which as a fact followed the works. I had the feeling that perhaps a 20 or 30% reduction might be obtained in the hospital returns . . . indeed I was prepared for a total failure of the works. . . . I record these feelings that they may encourage others who may be disinclined by the apparent magnitude of the task from attempting to combat this disease." The success was, however, complete and lasting.

Port Swettenham is situated on the river five miles from Klang. It was founded in 1895 in order to provide a port for ocean-going steamers. The country was covered with low-lying mangrove swamp frequently flooded by the sea. There were few inhabitants and few cases of malaria, but Watson observed that new arrivals quickly became affected. The place was practically a mangrove swamp in which about 15 acres had been cleared. On the 15th September 1901, the port was formally opened and the Government population and coolies connected with the shipping were transferred to it. Immediately afterwards "malaria assumed an epidemic character. In less than a month the 180 loading coolies were so decimated by disease that the remainder refused to live any longer at the port and returned to Klang." Freshly-imported coolies deserted,

Out of 176 Government servants, 118 were attacked between the 10th September and 31st December. Ships came in and could not discharge their cargoes. The outbreak was so bad that the High Commissioner ordered the closure of the port until it could be made more sanitary. In the meantime Watson had recommended measures similar to those taken at Klang, and a special commission appointed by Government adopted them. "Within six weeks the work of the port was proceeded with without great difficulty. An area of about 100 acres was bunded and drained and freed from jungle."

The results were immediate and decisive. The author had kept careful hospital statistics, including returns of all cases in individual houses, from facts known to him in his capacity as District Surgeon. The figures are given in a series of papers by him and Dr Travers (*Journal of Tropical Medicine*, 15th September, 16th November, 1st December 1903, 1st April 1905, 2nd July 1906). I attach the following abstract from my paper [1907].

1. *Cases of Malaria admitted to Klang Hospital from the Two Towns compared with those admitted from District*

Years	1901	1902	1903	1904	1905
Towns	610	199	69	32	23
District	197	204	150	266	353

2. *Deaths in Klang and Port Swettenham*

Years	1900	1901	1902	1903	1904	1905
Fever	259	368	59	46	48	45
Other diseases	215	214	85	69	74	68

3. *Deaths registered in District, excluding Towns*

Years	1900	1901	1902	1903	1904	1905
Fever	173	266	227	230	286	351
Other diseases	133	150	176	198	204	271

4. *Infected Children in Towns and District*

	Klang	Port Swettenham	District
November and December 1904			
Children examined	173	87	298
Children infected	1	1	101
November and December 1905			
Children examined	119	76	247
Children infected	1	0	59

5. *Sick Certificates and Sick Leave Granted to Government Employees*

(Numbering 176 in 1901 and 281 in 1904)

Years . . .	1901	1902	1903	1904	1905
Certificates . .	236	40	23	14	5
Days of leave .	1,026	198	73	71	30

Since 1905 the figures have not been accurately kept, but children examined at Klang show evidences of malaria as follows:—

1906, out of	142 children,	1	infected
1907 „ „	71 „	3	„
1908 „ „	455 „	13	„
1909 „ „	463 „	17	„

These are extraordinarily small percentages, considering that the district surrounding Klang was not fully treated. Meantime, the population of Klang had risen to an estimated total of 5,745. The results at Port Swettenham were not so permanent. The population spread into an undrained area, with the result that there was an outbreak in 1906. The cause of this was rapidly identified and removed by drainage.

The cost at Klang for the five years 1901 to 1905 amounts to $35,818 (the Malay dollar appears to equal 2s. 4d. British money). Watson says that "much money was spent in filling which could have been avoided had the town not been burdened by the legacy of its old brick drains," and there was other considerable waste. He adds that the drainage should have cost about £2 per acre, or about £800 instead of £3,000. "Thousands of acres in the neighbourhood have been cleared of malaria at this cost, and I can see no reason why Klang could not have been similarly dealt with at a similar cost," but of course many mistakes are made in first attempts. At Port Swettenham the cost amounted to $52,364. In addition to this capital expenditure there was an annual one of £270 for maintenance at Klang, and of £140 at Port Swettenham. In the former 332 acres (134 hectares), and in the latter 110

acres (45 hectares) have been dealt with. The total cost was about £1·2 per head of population for the whole five years together. The author says that in Klang "not one single grain of quinine was given to any of the population, except to those who were actual patients from malaria, in hospital or in my official or private practice." It should be observed that all this work was done before the species of carrier had been exactly identified.

(2). *District.*—The local Anophelines were identified by Dr G. F. Leicester in his Culicidae of Malaya in 1904. There are nine species, of which, according to Watson, two are certainly carriers. One of these, Mr Newstead tells me, is certainly *Myzorhynchus umbrosus*, though the genus is not quite certain, and the other is *Nyssorhynchus willmori.* The former breeds all over the country, and the latter only in the hilly tracts in quickly running streams. The former can be entirely removed by ordinary open drainage channels; the latter require subsoil pipe drainage, as it will breed with facility in the open channels.

Watson's account of the extensive malaria campaign in the district is a fascinating one, but as it is concerned with numbers of scattered rubber plantations, each of which has to be dealt with separately, the history is much too detailed to be given here, though that history is an example of what can be done in rural areas under the worst conditions by all the measures, especially by drainage. I therefore content myself with giving his conclusions and some of his remarks. The former are as follows:—

"(1) That both flat and hilly land in Malaya is, before opening, very malarious, and that blackwater fever has been found in both.

"(2) That hundreds of square miles of the flat land in Malaya have been freed from malaria simply by draining, and by felling the jungle.

"(3) That the cost of these rural anti-malaria measures in

Malaya (where labour is dear) is about £3 an acre, being £2 to drain and £1 to fell the heavy virgin jungle. This expenditure at the same time is the first step in agriculture, and under it the land has acquired a considerable increased value.

"(4) That this freedom from malaria coincides with the disappearance of an Anopheline (*M. umbrosus*) which breeds in undrained jungle, and does not breed in open earth drains when kept clear from weeds, and flowing.

"(5) That certain hilly land intersected by ravines, both open and drained, is as malarious as when first opened.

"(6) That the continuance of malaria here is due to *N. willmori*, which breeds in ravine streams, which cannot be driven from them despite the utmost care of keeping these streams free from weeds, and which cannot be completely washed out of the ravines even by the heaviest tropical showers.

"(7) That quinine given regularly greatly reduces the sick- and death-rate of those exposed to malaria.

"(8) That doses of less than six grains daily are of little value if the malaria be intense—say where the spleen rate is 75% or more.

"(9) That when given in 10-grain doses on six days out of seven, or in 20-grain doses when a coolie has fever, or is in such bad health that he does not feel inclined to work, between 20 and 30% of those taking the drug will still be found with parasites in their peripheral blood.

"(10) That the use of quinine can therefore never result in the abolition of malaria, or even make any material reduction in the liability to infection in a malarious[1] locality.

"(11) That mosquito-proof houses have a value; but the attempt to discover a satisfactory mosquito-proof coolie house ended as an inconclusive experiment.

"(12) That not only for urban, but for rural districts, anti-malaria sanitation should be based on mosquito reduction, if this be a physical possibility."

[1] ? Intensely malarious.—R. Ross.

The author has many additional remarks to make, of which I here note the following:—

(1) There was conclusive evidence that the construction of roads increases the malaria in Malaya by obstructing drainage.

(2) In the flat land removal of jungle within a radius of about half a mile from plantations suffices to remove malaria, as proved by many experiences over a distance of about 50 miles, but this does not suffice in the hilly land, where malaria is carried by *Nyssorhynchus willmori*.

(3) As shown by examination of large numbers of children, proximity to jungle greatly increases the malaria rate.

(4) He says, "We never seem to hear that a place always remains unhealthy, and never improves as time goes on. This is because, after a time, the population of an unhealthy place consists almost entirely of those who have acquired a certain amount of immunity. New people have practically ceased to come to it, and so the health seems to improve. But it only wants new arrivals to come in numbers to start a severe epidemic of malaria."

(5) The introduction of a large number of Tamil coolies has greatly increased the malaria rate in certain plantations. This is due to the principles laid down in section 30 (17 and 18).

(6) Regarding quinine prevention, he makes the following further remarks. On one estate fifty-six coolies were found to have parasites, though they were getting 6 to 8 grains of quinine, with double doses when they were ill. Of these 75% were apparently in the most perfect health, while 87% were in good health and fit for work. The author says that this "clearly demonstrated that, while keeping the malaria in check, so that the coolies can carry on their work, three years' administration of the drug had entirely failed to eradicate the disease. That the estate is really as unhealthy as ever is shown by the visitors to it who contract malaria in about ten days . . . finally, out of 19 *N. willmori* captured in the lines, 4, or 21%, had malaria, 2 with zygotes, 1 with sporozoits and 1 with both zygotes

and sporozoits." The author further examined an estate in which 10 grains of quinine were given in the most thorough and systematic manner to each adult who worked, and 20 grains to those who did not work, and 5 grains daily to children, with double doses when they were ill. The blood of 29 children was examined, and the parasites were found in 27.6% of these; while out of 125 adults the parasites were found in 18·4%, giving a total of 20% infected out of 154 persons. The author observes that quinine has little effect on the gametids; that patients die even after 40 grains of quinine daily, and repeats my teaching regarding the persistence of the sporids (Chapter IV); but adds: "If, as has been shown, the immunity from malaria produced by quinine leaves the patient infective while he is acquiring the immunity, then it will be impossible, in the presence of many Anophelines, and in the presence of many new arrivals (such as newly-born children), ever to eradicate malaria by quinine. It follows, too, that if drainage be an alternative, even though much more expensive, drainage must be the method which should be adopted. Even if the community possesses no money for drainage, money might be borrowed with which to carry out the works, and at the end there would be an asset to show for the expenditure. Borrowing, however, would be impossible if the object were to buy quinine. It must not be forgotten, too, that the administration of quinine in effective doses is by no means an inexpensive method. To give 10 grains daily to one thousand people, without any extra to those who actually have pyrexia, for ten years would cost about £1,900. Such a sum, if lent by a government to a community at a reasonable rate of interest, would free a very large area from malaria if drainage methods suitable to the local Anophelines were employed. And in ten years the community would have nine years of prosperity and health in which to repay the loan. In the case of many small villages, it would probably be possible to eradicate the breeding-places of Anophelines, where they are in the midst of the community, at a mere fraction of the

money which would be required to dose the population effectively with quinine, even for a year. While for other communities the cost of drainage would be relatively much less than in the case of the smaller ones, since the same expenditure would protect a relatively larger number of people" (section 39).

(7) The author carefully compares the conditions in Italy with those in his country. The reason for the freedom of the hills in Italy from malaria is that the principal carrier there is a pool breeder and not a stream breeder. The attitude of the Italians to quinine is due to local conditions, drainage being difficult in many malarious localities there, especially along the coast. But as regards the tropics, the author adds: "To me it seems only the vision of a dream that any organisation will ever induce a whole tropical population to take quinine in the doses required by the Italians. And when it is further considered that the population, even when taking this quinine, would still be infectious, the policy of spending money on quinine for an indigenous population, where drainage is possible, appears to me indefensible either on medical or financial grounds" (section 39).

(8) The author also discusses malaria in India, and criticises the statements of James and Christophers (*Lancet*, 20th June 1909). He does not agree with them that "prevalence of Anophelines, though always important, is by no means in every case the most important factor to be considered," or that the Italian measures are most suitable in India. He states that the Malayan experiences should apply also to India, and adds: "The logical conclusion therefore seems to me that quinine can never do more than give a temporary relief to India, and that the factor to be dealt with is the *Anopheles*, and that measures must be aimed at it—not only in towns, but also in rural districts." For *N. willmori* he advises closed subsoil drains.

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58. Prevention of Malaria in Formosa.—(1). *Brief sketch of Formosa.*—The island of Formosa, which came under Japanese rule in 1895, lies between Lat. 21° 45″ and Lat. 25° 37″ N., and between Long. 119° 18″ and 122° 6″ E., comprising an estimated area of 13,434 square miles. The following are the numbers of the population, according to the latest investigation:—

Formosans	3,036,855
Aborigines	120,254
Japanese	83,229
Foreigners (including Chinese)	12,151
Total population	3,225,589

Malaria prevails over nearly all localities of the island, and the deaths caused by malaria in 1906-1908 are given below.

Year.	Deaths from Malaria.	Total Deaths.	Per 1000.
1906	10,562	104,622	100·9
1907	11,015	102,853	111·9
1908	11,714	102,232	114·5

Statistics only for the years 1906-1908 are given above, no available ones being obtainable for the preceding years.

It must be remembered that there are still about 1,800 native practitioners of the Chinese school in the island, whose service is scarcely to be depended upon.

(2). *Mosquitos which carry the parasites.*—Seven species of

Anophelina are known to be in existence in Formosa, with an additional one recently discovered; they are as follows:—

(1)	*Anopheles*	(*Myzorhynchus*)	*sinensis* Wied.
(2)	*An.*	(*Myzomyia*)	*listoni* Liston.
(3)	*An.*	(*Myz.*)	*rossii* Giles.
(4)	*An.*	(*Myz.*)	*species*, from Taito.
(5)	*An.*	(*Nyssorhynchus*)	*annulipes* Walk.
(6)	*An.*	(*Nys.*)	*maculatus* Theobald.
(7)	*An.*	(*Nys.*)	*fuliginosus* Giles.
(8)	*An.*	(*Cellia*)	*kochi* Dönitz.

Of these, the first two species have been found to be the medium of malaria. According to the announced result of experiments made by the late Dr Kinoshita and Dr Miyajima and others, *Anopheles sinensis* has the power of developing the *Plasmodium vivax* of tertian fever, but not the *Plasmodium praecox* of aestivo-autumnal fever; while *Anopheles listoni* is host of *Pl. praecox*, its cysts growing upon the mosquitos' intestinal wall. In other words, tertian fever is transmitted by *Anopheles sinensis*, and aestivo-autumnal fever by *An. listoni*. *Anopheles rossii* being recognised as an innoxious species, no cyst of *Plasmodium* can be found growing in its body by process of infecting experiments. The pathogenical importance of other Anophelines are not yet clearly defined.

In the order of the extent of prevalence, *Anopheles sinensis*, the tertian fever carrier, comes first; and next comes *An. listoni*, a much smaller but extremely dangerous species. *An. rossii* is mostly found in the middle and south of Formosa, its sphere of influence in the north being limited to 24° N. The newly-discovered *Anopheles* from Taito (on the east coast) was likewise collected in the southern part. The remaining three species of *Nyssorhynchus* are met with in the mountainous districts. The newly-discovered *An. kochi* has been obtained in Keelung, which lies 18 miles north of Taihoku.

(3). *Preventive measures adopted.*—

A. By the general improvement of sanitary conditions in towns and in the country, as, for instance, by the construction of properly macadamised and paved roads, the discontinuance of the use of the drinking-water mixed with sewage, the facilitating of the carrying off of rain-water and drainage, and so on.

B. Protection from mosquito attacks by means of wire-gauzed shutters and windows, and mosquito-nets, etc., etc. This means is chiefly adopted by the garrison troops here.

C. Prophylactic administration of quinine after R. Koch.

D. Thoroughgoing post-malarial treatment.

(4). *Results.*—As a marked result of the above-mentioned public measure *A.*, Taihoku and Taichu, which formerly used to be malaria-stricken places, are now found completely cleared of the dreaded disease. As to the merit of the mechanical measure *B.*, reference may be made to the uniformly improved health condition among the troops of Formosa. The measure of screening the barrack windows, etc., with wire-gauze was first resorted to by the garrison here in the summer of 1909.

The following Table shows its result:—

TABLE I.

Year.	Malaria cases per 1,000 soldiers.	Malaria deaths per 1,000 soldiers.
1897	2724·3	17·3
1898	2493·9	19·3
1899	2351·6	23·9
1900	2345·5	20·7
1901	1768·2	11·5
1902	1142·7	7·1
1903	460·8	1·7
1904	213·8	1·1
1905	145·0	3·1
1906	280·3	2·1
1907	528·1	2·1
1908	498·3	1·3

The unusual increase of the number of malaria-affected soldiers in the last year is accounted for by the fact that in the said year many soldiers fell victims to malaria during the big campaign against the savages, and that the sick-rate adopted by the Army was rather too high.

As a result of the medicinal measure *C.*, the experiment made at Kosenpo may profitably be referred to.

Kosenpo is a village lying in the southern mountainous districts, the majority of whose inhabitants are camphor collectors.

In April 1907 malaria prevailed terribly in this locality, especially among Japanese new-comers, so that 30% of the Japanese residents proved to be malaria patients. From July 1907 R. Koch's gramme-prophylaxis began to be systematically used under the direction of the late Dr Kinoshita. The people were administered quinine hydrochlorate in pastil, in *pro dosi* 1·0 for adult, every nine and ten days successively. The prophylaxis was strictly conducted under care of the police officials from July to November 1907, and from January to October 1908. The reduction of both malaria-morbidity and malaria-mortality will clearly be seen from the following Table II.

(5). *Estimated cost incurred for the prevention of malaria.*—No estimated costs for the prevention of malaria can be given, as they differ according to various circumstances.

NOTE BY R. ROSS.—By the kindness of Professor Dr Kitasato, Dr M. Miyajima of his Institute for Infectious Diseases at Tokio has supplied me with the malaria statistics of the Imperial Japanese Army, serving in Japan, China, Chorea and Manchuria, as given by the Medical Bureau of the Army. The totals for three years are as follows :—

Years.	Total Patients.	Total Deaths.	Malaria Patients.	Malaria Deaths.
1906	77,100	749	2,717	10
1907	104,266	636	4,863	9
1908	114,794	646	4,666	7
	296,060	2,631	12,246	26

Dr Miyajima says that in Japan itself the parasite is almost exclusively *P. vivax*, but that the other species occur in Formosa and the Loo Choo Islands. The largest number of malaria cases were in Formosa and in Chorea (5,348 and 1,974 cases respectively in the three years).

TABLE II.

Year.	Month.	Japanese Inhabitants.	Malaria cases.	Malaria deaths.	Malaria cases per 1,000 Japanese.	Malaria deaths per 1,000 Japanese.
1906	Jan.	314	Unknown	Unknown		
	Feb.	211	51	1	265·4	4·74
	March	387	105	1	271·3	2·58
	April	502	165	1	328·7	1·99
	May	741	256	2	345·5	2·70
	June	754	394	1	522·5	1·33
	July	836	408	7	488·0	8·37
	Aug.	1,006	593	9	589·5	8·95
	Sept.	984	503	12	511·2	12·20
	Oct.	1,094	659	6	602·4	5·48
	Nov.	1,229	579	14	471·1	11·39
	Dec.	1,364	496	5	363·6	3·67
	Total	Average Population 785·2	4,209	59	5,361·8	75·14
1907	Jan.	1,494	329	1	220·2	0·67
	Feb.	1,435	244	1	170·0	0·70
	March	1,508	172	3	114·1	1·99
	April	1,626	220	0	135·3	0
	May	1,708	243	1	142·3	0·59
	June	1,806	274	2	151·7	1·11
*	July	1,487	208	1	139·9	0·67
*	Aug.	1,588	141	2	88·8	1·26
*	Sept.	1,604	76	1	47·4	0·62
*	Oct.	1,625	46	1	28·3	0·62
*	Nov.	1,668	75	2	45·0	1·20
	Dec.	1,665	116	2	69·7	1·20
	Total	Average Population 1,601·2	2,144	17	1,338·1	10·61
1908*	Jan.	1,669	102	0	61·1	0
*	Feb.	1,668	60	1	36·0	0·60
*	March	1,675	44	0	26·3	0
*	April	1,674	58	0	34·6	0
*	May	1,883	92	0	48·9	0
*	June	1,918	127	0	66·2	0
*	July	1,898	169	0	89·0	0
*	Aug.	1,933	152	0	78·6	0
*	Sept.	1,949	168	2	86·2	1·03
*	Oct.	1,829	183	2	100·1	1·09
	Nov.	1,282	169	3	131·8	2·32
	Dec.	1,258	143	1	113·7	0·79
	Total	Average Population 1,719·2	1,467	9	818·2	5·23

(* indicate prophylacted months.)

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59. Notes on Some Other Works. — It is impossible to give within the limits of this book a full account of all the campaigns which have been commenced, or even to mention some of them; but I think that brief notes on the following works should be added.

(1). *India.*—In section 7 I described my own work in India up to the publication of a report [1899] in which I suggested my proposals for mosquito reduction. By this time I had left India; and no notice was taken of the report, except that several medical men wrote against the idea. They pointed out that Anophelines breed in rice-fields, and so on, near cantonments, and therefore thought that the task of banishing them would be impossible. They evidently failed to understand my arguments, and had no idea of the economical principle laid down in axiom 3 of section 38.

At this time many persons were writing up the recent discoveries. Unfortunately, some of them were not personally acquainted with the tropics, and few of them had any knowledge whatever of practical tropical sanitation. It was almost impossible to make them understand my proposals; but some of them clamoured for a formal experiment to test them in the field. I was very averse from this suggestion. Success would demand a long and patient enquiry, requiring an exact preliminary survey of the amount of malaria and the number of mosquitos present, followed by equally exact measurements of these quantities made on frequent occasions. Now it will be seen from Chapter V how difficult it is to measure malaria

exactly, and how much more difficult to obtain any direct enumeration of the Anophelines. Moreover, failure might be due, not only to the impracticability of my proposals, but also to want of enthusiasm or care in those who were appointed to test them. Thus failure, or at least an indecisive result, was very probable, and would have a disastrous effect upon anti-malaria work for many years. I thought it would be much better to allow local authorities to attempt the work quietly in the ordinary course of sanitation, and not as a formal test experiment; and this was actually done since 1902 in Havana, Ismailia, the Federated Malay States, and elsewhere.

But my critics could not be got to understand the bad policy of their suggestions, and consequently the so-called test experiment was commenced at Mian Mir, near Lahore, Punjab, India. The place was a most unsuitable one; almost flat, with an impervious subsoil and watered by irrigation, mosquito reduction was not easy in it. Moreover, it was a military cantonment containing scattered barracks, from which statistics of the local malaria rate could hardly be compiled, since the troops and their followers were frequently being changed from other stations; while, lastly, the place was scarcely a town, that is, a locality which was most appropriate for mosquito reduction. In fact, the selection of the place showed how little the subject was understood at that time.

Mian Mir was examined by the Malaria Commission of the Royal Society in 1901, but without any exact estimates of the amount of malaria or the number of Anophelines. In April 1902, however, practical operations were at once commenced, evidently on the model of my suggestions for Sierra Leone, and were continued for that and the following year. For 1902 the operations were reported upon by S. P. James [1903], and for the following year by S. R. Christophers [1904]. The former reported very doubtful success, and the latter practically denied the feasibility of mosquito reduction. Their reports were, however, exhaustively criticised by myself in a paper

[1904], to which no reply has been given; and also by Sewell [1904], Giles [1904], and others.

No one who is acquainted with the nature of scientific evidence can imagine for a moment that this experiment gave any proof of anything connected with the subject. The measurements of malaria were of the most inadequate nature, and were based upon the most insufficient random sampling —while in some cases the number of children examined was not recorded (section 31 (8)). The enumeration of mosquitos was evidently of an equally casual nature, the authors not having been able to devise any accurate method. Neither report was well written, and the second one was worded so confusedly that almost anything might be inferred from it. Both reports claimed that the mosquitos increased in spite of the reduction measures. This is, of course, possible to a certain extent (section 29 (8)); but it is otherwise mathematically impossible, and the statement suggests only that the authors' methods were inaccurate. As no exact enumeration of the mosquitos had been made before the operations, the total result of the work could not be determined. In fact the reader who is interested in exact work will do well to compare these reports with Chapter VI, if only to learn what not to do as regards the measurement of malaria and of mosquitos, and the conduct of a campaign.

It has been frequently claimed by several Indian writers on the prevention of malaria, that the operations at Mian Mir were carried out merely to determine the practical feasibility of mosquito reduction for a reasonable cost in India. Even this claim cannot be accepted. During the first year (1902) only 7,216 rupees were expended for dealing with an area of 4 square miles containing numbers of troops, and of this sum, 5,364 rupees were spent on bricking, lining and plastering a single watercourse, leaving only 1,851 rupees, or about £120 for the rest of the work. This is hardly a convincing expenditure for a test experiment. The life of a single soldier

costs more. It will scarcely be believed that in the second report of the experiment, designed, as has been declared, for the express purpose of providing official information for the whole of India, no accounts whatever of the cost incurred were vouchsafed! In fact, the whole affair was conducted on unpractical and unscientific lines. It proved nothing at all, and its only effect was to retard anti-malaria work in that and other countries for years.

Since 1904 the military authorities at Mian Mir undertook much more patient and useful work there, with the apparent result of a considerable reduction in malaria — see especially H. D. Rowan [1908]. At the Bombay Medical Congress of 1909, however, S. P. James described how, during a recent visit to Mian Mir, he had found no reduction, either in the malaria or in the number of mosquitos. His statements were immediately controverted by five administrative medical officers (see *Lancet*, 3rd July 1909). James stated that out of ten men of the Gloucester regiment, who had been taking quinine regularly, and who were selected by him at Mian Mir "more or less at random," he found parasites in no less than eight—an enormous percentage. The commandant of the regiment, however, informed me at Bombay shortly afterwards that his regiment had not suffered much from malaria at that time.

Many local campaigns have been commenced in India, and will be found mentioned in the Annual Sanitary Reports, in the Proceedings of the Malaria Conference [1910], and in a recent publication by W. G. King [1909], and elsewhere. I have received many private communications regarding these, from which I gather that they have not been given much encouragement from headquarters, that the organisation has not been sufficient to ensure continuity, and that the necessary measurements of malaria have been quite inadequate. There is no doubt that the general policy of a *clique* in India has been opposed to mosquito reduction, and has not been very keenly interested in any other form of malaria campaign. A

party in that country has been advocating quinine on the Italian model, and appears to have persuaded the Government that it will cost nothing, while mosquito reduction must always be enormously expensive. Personally, I would welcome any measure that is effective, but apparently not even the quinine method has been fully pushed in that country. Experience will, I think, prove to the Government of India that no anti-malaria campaign can be conducted without expense; that each measure has its proper sphere of action; and that quinine may not always cost less than other measures. Up to the present the work in India seems to me to have been wanting both in intelligence and in leadership, in spite of the 1,000,000 deaths a year officially attributed to malaria there.[1]

In October 1909 an Imperial Malaria Conference was held at Simla. It was not, I think, arranged in a perfectly satisfactory way. Engineers and several good workers on the subject appear not to have been called. Nevertheless, the conclusions of the Conference simply confirmed my proposals, especially as put in my Mauritius report. I hope that the Conference will mark the beginning of a new era in India.

C. A. Bentley has recently published a report on malaria in Bombay [1910]. A. G. M'Kendrick had found a splenic index of $629/8325 = 7{\cdot}5\%$, and the author one of $3075/21{,}517 = 14{\cdot}3\%$. The local carrier is *Nyssorhynchus stephensi*, which, unlike most Anophelines, breeds readily in deep wells, cesspits, cisterns and salt water. The author recommends a campaign against it (not including other mosquitos) chiefly by means of strict legislation aimed at the removal or protection of such waters by householders, and does not appear to advocate any special municipal organisation. Such legislation is certainly a necessary preliminary and easy to suggest; but in practice the cost of the numerous inspections and summonses required to enforce it is apt to exceed that of doing the work by departmental agency, as hygienists well know (axioms 6 and 7, section 38). Nevertheless, municipalities like the idea, because

[1] See also section 65 (11).

it appears at first sight to put them to no expense. We generally observe two things about tropical municipalities—the excellence of their sanitary laws and the completeness with which the public ignores them. Which is the cheaper in the end, (*a*) to make one inspection and then do the work, or (*b*) to make many inspections, worry the householder, issue several summonses, be finally forced to do the work, and then try to recover the cost in the law courts? We must always remember this question, and endeavour to arrange a proper balance between the respective obligations of the householder and the municipality.

(2). *Hongkong.* — One of the earliest and best of the campaigns in British territory. The city of Victoria, usually called Hongkong, runs for nearly 5 miles along the north of the island of that name at the mouth of the Canton river in South-East China. The island, 11 miles long and from 2 to 5 miles broad, consists of a broken ridge of hills, rising to nearly 2,000 feet, and the city is built on a hill sloping down to the water, some of the terraces and houses being 500 feet above sea-level. There is also a large residential district on the mountains reached by a cable tramway. The soil is granitic. All along the face of the hill on which Victoria is built there are beds of streams, known as "nullas," which used to swarm with Anopheline larvae. The population of the colony was 377,850 in 1905, of which 10,835 were whites (nearly half belonging to the British Army and Navy). The rainfall is from 70 to 80 inches a year. Malaria has been always very prevalent here, and I remember that in 1881 the colony was cited as an example of the telluric miasma due to decaying granite. The first researches on the new lines were commenced as early as May 1901 by Dr J. C. Thomson [1901], who undertook an exhaustive study of the mosquitos and their breeding-places. He examined over 32,000 specimens, of which he found about 4% to be Anophelines, and in November advised an active anti-malaria campaign by drainage,

clearing of jungle, "training" of the nullas, the use of wire-gauze, oiling pools, and quinine prophylaxis. As seen by his excellent papers [1900-1903], his recommendations were not of a general nature, but were particular, practical and exact. These recommendations were rapidly acted upon by the Government. Since 1901 all the nullas or watercourses within and near the city were "trained"—that is, rendered so smooth and even that the Anophelines could no longer breed in them; and much similar work was done wherever most needed elsewhere by training watercourses, buying up rice-fields, and so on. The details of the campaign are so numerous that it is impossible to give them here. They will be found in the publications given in the bibliography and in a good paper by Mr J. M. Young [1901], who took part in the early stages of the work. The results are given in the annual sanitary reports of the colony, and in a recent address by the medical officer of health, Dr Francis Clark [1906]. Dr Thomson informs me that before estimating them it is necessary to remember that malaria can never become extinct in Hongkong owing to the fact that some three thousand to four thousand natives come and go from and to the country districts every day, and that a number of these will remain infected in spite of all local measures. Nevertheless, the figures show a rapid diminution both in the admission and in the death-rates.

Malaria Statistics in two large Hospitals

Years	1897	1898	1899	1900	1901	1902	1903	1904	1905
Admissions	1021	865	780	1,220	1,294	752	568	433	419
Deaths	197	126	63	163	132	128	63	58	54

Admission Rate of Police for Malaria

Years	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905
Admissions %	32	25	19	31	42	44	19	18	11	12

Deaths from Malaria

Years	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905
Population	239,419									377,850
Total deaths	533	554	530	546	555	574	425	300	301	285
Deaths in city (Chinese only)	290	302	280	218	242	281	189	152	90	87

The official sanitary reports give similar figures. The improvements have, of course, varied much in different localities. Thus in 1900 the western end of Bonham Road used to be one of the worst districts. Now, in 1905, it is reported not to have sent a single case to the Government Civil Hospital [1900-1905].

With regard to cost, Dr Clark reports that up to the end of 1905 the Government had expended about £5,000 on anti-malaria measures, and estimates that £6,500 would be spent by the end of 1906—a small amount to pay for the good that has been done. The campaign in such a thickly populated district must be difficult. Figures for subsequent years have not been obtainable.

(3). *Candia, Crete.*—The accounts of this excellent campaign, conducted in connection with the British troops in Crete, will be found in successive numbers of the *Journal of the Royal Army Medical Corps.* The town of Candia, containing 21,000 inhabitants, is situated on the seashore of a semicircular plain bounded by hills. The rainfall is heavy, and the ground is traversed by many streams, which become torrents in winter and nearly dry in summer. Two of them have marshy margins near the sea. The houses of the town are of the Oriental type and contain many wells. The troops are accommodated in quarters to the west of the town and close to it, and suffered considerably from malaria. Apparently as early as 1902 Lieutenant-Colonel J. V. Salvage, R.A.M.C. [1904], commenced to examine into the distribution of Anophelines and to destroy the larvae by drainage, or by closing or oiling the wells, or putting fish into them, and expressed himself hopefully regarding the result. In March 1903 Major C. J. MacDonald arrived in Crete and continued the work [1905]. He gives many very interesting details. Works of a permanent nature were attempted, and quinine prophylaxis and mosquito-nets insisted upon. The first results were as below:—

Years	1901	1902	1903
Strength	564	460	410
Admissions	1,540	1,084	227
Rate %	273	236	55

The admissions include simple continued fever, together with malaria, in order to avoid error due to diagnosis. Major MacDonald attributes the fall to the measures taken. In 1905 Captain R. A. Cunningham, R.A.M.C., adds a third article [1905], in which he stated that the fall in the malaria rate had continued, and was only 30% in 1904 compared with the 55% of 1903. The mosquitos were so few that it is almost unnecessary to use a net at night. In 1905 there were 246 admissions, or a rate of 38·7%. Many men become infected on guards and outposts.

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60. The Prevention of Malaria in War.—*Part I. The Lessons of History.*—The history of malaria in war might almost be taken to be the history of war itself, certainly the history of war in the Christian Era. Even in Europe up till at least the middle of the nineteenth century all armies in the field were liable to attacks of this class of disease, as witness the French and Austrian armies in the campaign in the north of Italy in the year 1859. It is probably the case that many of the so-called camp fevers, and probably also a considerable proportion of the camp dysentery, of the wars of the sixteenth, seventeenth and eighteenth centuries were malarial in origin. In our own army, as the writings of Pringle and Monro teach us, the various campaigns in the Low Countries were marked by severe outbreaks of paludal fevers, culminating in the epidemic that wrecked the ill-fated Walcheren expedition just one hundred years ago. It would be out of place here to do more than mention these earlier histories, but the Walcheren disaster merits more than passing notice, for several reasons. In the first place, it occurred in comparatively recent years. There must be men still alive who can remember having seen and spoken to survivors of the expedition, some, at least, of whom must have carried the marks of their sufferings to the grave. In the next, the close proximity of the scene of action

to our own shores struck even contemporaries with a sense of the reality of the disaster. These two characteristics tend to make this outbreak one of peculiar interest; but there are others of even greater value than interest, which bear very closely on the question of prevention of malarial fevers in campaigns of the present day. Of these the first is that the locality selected for the debarkation of the troops was one notoriously unhealthy on account of malarial fevers. Already, in 1747, the force landing in Zealand had suffered to such an extent from this cause that, according to Sir John Pringle, some of the battalions in South Beveland and Walcheren had but 100 men fit for duty, being less than one-seventh part of their whole number. Another experience repeated in the expedition of 1809 was that the men-of-war which lay at anchor in the channel between South Beveland and Walcheren, even during the worst period of the distemper, were not affected with either flux or fever, but enjoyed the most perfect health. In the light of our present-day knowledge this coincidence is easy of interpretation; but the practical lesson to be drawn was clear, or should have been clear, even in 1809, namely, that on a malarial coast troops should not be landed from the transports until all steps are ready for an immediate advance. It was the preliminary delay under the walls of Flushing that laid the army of Lord Chatham open to the first onslaughts of disease. An early advance into the comparatively higher ground of the interior would undoubtedly have done much to check the disease. In the second place, the season of the year selected for the expedition was notoriously the most unhealthy of the year. And this fact also was on record. Sir John Pringle wrote as follows in the year 1765: "The epidemics of this country may therefore be generally dated from the end of July, or the beginning of August, under the canicular heats: their sensible decline, about the first falling of the leaf; and their end, when the frosts begin." Here, again, the lesson was as plain as the warning. If a force has to land in a malarious locality then the unhealthy season

should be carefully avoided. How long it took before that lesson was learnt the records of our earliest campaign in Ashanti, and the experiences of the French in Madagascar, show only too clearly. And lastly, there was no selection of the men. Davis, who was appointed to attend the troops on their return to England, in his "Scientific and Popular View of the Fever of Walcheren" (p. vii.), alludes to the fact that the troops consisted of "numbers of young men unaccustomed to fatigue and the hardships of a military campaign." It may be accepted as one of the most important points in the conduct of a campaign in a malarial locality that the men should be seasoned soldiers, free from the taint of previous severe malarial infection, but accustomed by experience to the proper camp discipline essential to the prevention of paludal diseases.

The expedition left the shores of England on the 28th July 1809, and disembarked in Middleburg and Walcheren on the 29th and 30th of the same month. The siege of Flushing was at once undertaken, and this place surrendered on the 15th. Up till that date, with the exception of the men killed in action, there had been no deaths in the force of 17,000 men, and though the medical authorities on the spot were justifiably anxious, in view of the medical history of the locality, the general public were far from sharing in this apprehension, "observing that the notion of the unhealthiness of Walcheren being so great that it ought not to be garrisoned by British troops, was too absurd to deserve much notice; as it might probably be as unhealthy as Romney marsh, or the hundreds of Essex."[1] This was on the 23rd August, but "a different impression was felt in Walcheren, for as early as the 25th it appears, by a letter from the expedition, that the interruption to active operations which had then taken place from the rapidity with which the disease was spreading had caused much regret."[2] By the 29th August there were 3,000 men in hospital out of a total of about 25,000 men. Early in September there

[1] Davis, p. xi. [2] *Ibid.* p. xi.

were upwards of 7,000 men in the different hospitals, and by the 14th it was estimated that out of a strength of 15,000 in Walcheren, 10,000 were actually sick, the deaths then averaging at from 25 to 30 per day. The repatriation of the troops to England had already begun, and "about the 13th they began to arrive at Deal, where soon after the mortality in one hospital amounted to 14 in one day . . . indeed the mortality among those who first arrived in England was greater, comparatively, than at the same period even in Flushing, for there the whole daily mortality, though seldom less than 50, for some time previous to the 12th had never on any one day exceeded 100."[1] At the latter place, "on the 23rd of September, the sick report included 218 officers, 382 sergeants, 190 drummers and 9,046 privates."[2] From that date the fever subsided. On the 26th there were 9,798 in hospital, and on the 30th 8,600, and though the army was disabled and vanquished, its invisible enemy gradually slackened its pursuit, and the "remnant of an army" returned to England on the 23rd December, having faced fifteen days' fighting with 247 casualties, and two months' fever with more than 4,000 dead. And how about the wounded in this appalling struggle? Years afterwards the men who survived still showed in shattered constitutions the results of the ineptitude which planned, and the inefficiency which led the expedition of 1809.

The expedition to Walcheren is one of the most striking instances of the part that epidemiology should play in strategy. There were, on political and military grounds if on no other, doubts as to the advisability of attacking the French in the Low Countries. The known unhealthiness of Zealand should have been recognised, and should have acted as the decisive factor. In whatever part of the French dominions a British force had been landed at that season of the year, the neighbourhood of Walcheren was the last that should have been selected.

[1] Davis, p. xvii.

[2] *Ibid.* p. xviii.

In no other campaign during the "Great War" did malarial fevers play so commanding a *rôle* as in the expedition of 1809, though it is true that both in Estremadura and before Cuidad Rodrigo, the army under Sir Arthur Wellesley suffered notably from this type of disease. The chief interest, however, of paludism attaches historically in the nineteenth century to campaigns in the tropics, and as this will undoubtedly be even more the case in years to come, I do not propose to refer in this brief sketch to any other class of enterprise. And I will further limit myself to a consideration of two of our own expeditions to Ashanti, and to that of the French to Madagascar in 1895, since these present typical instances of small wars where success or failure depended not on the bravery of the men, or the tactical skill of their leaders, but entirely on the amount of forethought paid to the problems of disease at home, and to the translation of that forethought into terms of energetic action on the spot.

Passing over the earlier unsuccessful, or only partially successful campaigns of 1822, 1824 and 1833 in Ashanti, I shall begin with that of 1864. Of this expedition the late Surgeon-General Gore (sanitary officer with the expedition of 1873) writes ("Medical History of our West African Campaigns," p. 10): "It can scarcely be called a war, as an enemy was never seen, or a grain of powder expended; our troops were defeated by disease, much of which was preventable." He goes on: "In August of the former year (1863), the right wing of the 4th West India Regiment landed at Cape Coast Castle. Preparations were at once commenced for the future advance. Stores were collected and paths cut, these operations lasting until December, during the continuance of which the men suffered much from dysentery, remittent and intermittent fever." This point is worth noting. One of the most essential precautions in the case of tropical campaigns is to see that the collection of stores is carried out in anticipation of the landing of the expeditionary force. The campaign, as already stated,

was a fiasco. By the end of March one quarter of the strength was in hospital, and by the middle of May all the members of the original force at Prahsue were relieved. The admissions in 1864 amounted to over 1,600, and the deaths to a fraction under 60 per 1,000. It must be noted, too, that these troops were not Europeans, but black troops from the West India regiments. Of 60 European officers 35% succumbed to the effect of disease. The lessons of this campaign were not, however, lost.

Early in 1873 the Ashantis made one of their periodical inroads into the Gold Coast Protectorate. Owing to the scanty numbers and scattered distribution of the British garrison, our men were confined to the defence of the posts held by them. A small reinforcement of Marines was despatched from England, and reached Elmina, the most seriously-threatened post, by the 12th June, inflicting on the next day a serious repulse on the enemy. Affairs remained *in statu quo*, the enemy becoming somewhat less active, but still maintaining a threatening attitude, until, in September, an attack on the boats of the *Rattlesnake* demonstrated that sterner measures must be taken to deal finally with them. Sir Garnet Wolseley arrived from England on the 2nd October, and at once began the erection of wooden huts, and the enlistment of native artificers, labourers and porters, as well as the enrolment of local levies and regiments under specially-selected officers, "it having been thought that with European organisation and guidance Ashanti armies might well be successfully encountered by Africans of other races." [1] The first blow was struck on the 14th October, when a force advanced from Elmina and captured and burnt the village of Essaman, with but trifling loss. As a result of this blow the Ashantis prepared to retreat to their own country, but Sir Garnet decided that to complete their punishment a force containing a strong European contingent should be sent into Ashanti territory. In spite of well-founded apprehension as to the consequences of employing European

[1] A.M.D. Report, 1873, p. 232.

troops in the climate of the West Coast, it was decided that a force corresponding to the requirements formulated by General Wolseley should be despatched, it being considered, as a result of the experience of campaigns in low-lying, swampy districts of other tropical countries, that "there was a season of comparative healthiness corresponding to the driest season of the year, and that in this, with suitable precautions and limitations, Europeans might undertake field service in the Gold Coast country."[1]

Pending the arrival of the European force operations were undertaken with the troops at hand with a view to clearing the enemy from the territory of the Protectorate, and facilitating the advance of the punitive force as far as the banks of the Prah. Sites were selected between the base at Cape Coast and the river named, for the formation of camps, temporary barracks being erected at these places. In all, eight such sites were fixed, the average distance between one camp and the next being a fraction over nine miles. The object in view was to ensure that "the troops were to march into stationary camps, and to reach the Prah—the second base of the expedition—as little fatigued as possible, so that the force might start thence with the men in the highest state of efficiency, and strike the intended blow before it was materially reduced by sickness."[2] Eventually, the first detachment of European troops landed on the 1st January 1874, and the whole strength was assembled and the river crossed on the 21st. Some rather smart fighting occurred at Amoaful on the 31st January, and again outside Coomassie on the 4th February. The city was entered that same evening, and burnt and evacuated on the 6th. The entire force, with the exception of some of the irregular corps, recrossed the Prah by the 14th February, that is, within three weeks of the date of first crossing it. The 42nd Highlanders, which was the first regiment to land, was also the last to leave. The period that elapsed between landing and embarkation was eight

[1] A.M.D. Report, 1873, p. 236. [2] *Ibid.* p. 246.

weeks all but one day. The admission rate was 58 per 1,000 for European rank and file, and the death-rate 18·2 per 1,000. Five British officers and 13 men were killed in action, while 21 and 40 of these two classes respectively died of disease. This striking contrast between the two campaigns of 1864 and 1874 deserves to be noted. The keynotes of success in the latter case were careful preliminary preparation, and swift and rapid execution. Failure in the former expedition was due to "unselected men, under officers many of whom had already suffered from illness, not especially well equipped or cared for, remaining in the field at a bad season, and languishing in inaction month after month."[1]

The only other historical instance on which I shall dwell is the French campaign in Madagascar in 1895. The necessity for an expedition to Madagascar was recognised in the late months of 1894, but already for a year prior to the actual parliamentary sanction for the despatch of a force, the Naval and War Departments had had such a possibility under consideration, and the former had, with commendable forethought, caused reconnaissances to be made of the possible points of disembarkation, and of the best lines of advance. In August 1894 an inter-Departmental Committee was formed at the Foreign Office, in which the two departments already named and the Colonial Office collaborated in the production of a joint report on the geographical and military situation, with suggestions as to the composition of the expeditionary force, and the various measures to be adopted to meet the grave difficulties presented. There was in fact no want of preparatory consultation. About the middle of November it was decided to entrust the execution of the plans to the Ministry of War, and accordingly a fresh commission was formed, of an advisory nature, which sat at the War Office under the direction of the Chief of the General Staff, and was presided over by General Duchesne, specially selected for the chief command of

[1] A.M.D. Report, 1837, p. 237.

the expeditionary force. On this commission all departments concerned were represented, and a second commission entrusted with a fuller consideration of details was also appointed.

Meanwhile time passed. It was necessary to start active operations by May at the latest, and in fact the advance guard of the expedition landed at Majunga twenty-four days before the last-named commission completed its work. Even so the War Office was by no means independent in its management of affairs. Diplomatic arrangements still remained under the Foreign Office, while the troops available came partly under the Ministry of Marine, partly under the Colonial Department, and partly under the War Office. As one result of the complication of authority and responsibility the force was compelled to march for forty-three days through a malaria-infested country, instead of proceeding by steamer, though the necessity for the latter expedient had been recognised, and arrangements to that end begun. Transport being deficient and unsuitable, the troops were forced to advance in heavy marching order, in spite of the heat. Medical equipment was left behind, and even quinine was often not to be had. No special hutments were provided, and Malagasy huts had to be utilised even at the very base.[1] "From the 1st to the 24th March the troops were almost entirely occupied in disembarking *matériel*, in building shelters and making roads, work made all the more laborious by the lack of native labour, and the heavy rain." Just as there was no lack of forethought at home, so there was no lack of energy at the front. Unfortunately, the divorce between the two was complete.

Eventually the supply of water at Majunga ran short, the apparatus for distillation being insufficient, and an advance had perforce to be made. One company of engineers that left Majunga 225 strong, arrived at Suberbieville with only 25 worn-out men still effective. It is unnecessary to prolong this narrative. Reynaud gives in his excellent "Considerations

[1] Reynaud, p. 312.

Sanitaires sur l'Expédition de Madagascar," a detailed account of the misfortunes that continued to befall the expedition. From Suberbieville a flying column was despatched to Antananarivo, which it reached on the 30th September after incredible exertions, leaving ten per cent. of its strength on the road. The French soldiers christened this column "la colonne Marche-ou-Crève," a not inappropriate expression for a force where to fall out meant mutilation and death at the hands of a cowardly enemy. And yet many fell out and remained to die of starvation, worn out with suffering and illness. And as Dr Reynaud reminds us, this was the march of a conquering army, not a precipitate flight before a victorious enemy, but the culminating point of an expedition, the details of which had been carefully thought out. The last remnants of this unfortunate army left the shores of Madagascar on the 28th December, leaving behind them 4,498 dead out of the combatant force alone. Amongst the auxiliaries it was estimated that there were 3,000 deaths, whilst as regards non-combatants the numbers are beyond calculation. The proportion of deaths amongst the combatants was equivalent to 320 per 1,000 of strength, due almost entirely to malarial fevers and their *sequelae*. The deaths in action were 13, and in addition there were 88 men wounded. The causes of this disaster were the antitheses of those which led to our success in Ashanti in 1874 — confused councils, divided authority, and delayed execution.

Part II. Practical Measures.—We now come to the important question of the prevention of malarial diseases in war. I propose to discuss this subject under two heads. First, the measures to be adopted in anticipation of the opening of hostilities, and secondly, those to be carried out during the actual progress of active operations.

Measures to be adopted in anticipation of the outbreak of hostilities.—Before actually detailing these, it is necessary to insist on their enormous importance. It is not going too far

to say that the success of any tropical campaign depends infinitely more on the administrative measures taken in the preparation for the expedition than on the courage and fighting capacity of the troops engaged. The success of the Ashanti campaign of 1874 was ensured in Pall Mall before ever Sir Garnet Wolseley left England, and the fate of the French expedition to Madagascar in 1895 was decided in Paris and not at Majunga or Suberbieville. The only serious problem in a tropical campaign is disease. This has been proved time and again. No soldier will for a moment pretend that either in Burma or Ashanti the fighting was of a serious nature, and the same is true of the French army in Madagascar. The only obstacle to be feared was disease, and most importantly malaria. Now there is this difference between a human enemy and disease. The former may alter his plans at the last moment, and our plans may have to be changed accordingly. But disease does not alter its laws. As far as we know or can guess at those laws we are able to calculate confidently on their regularity, and our plans can be fixed accordingly. It is almost a platitude to say that the success of any tropical campaign is settled as much in Whitehall as on the actual spot. An ill-provided, ill-thought-out expedition can only end in disaster, however brave the soldier, and however skilled his commander. On the other hand, but little military skill and no extraordinary amount of courage is necessary to enable a small picked force of healthy Europeans, armed with modern weapons, to march through the territory of the most powerful savage tribe in the tropics. But what is necessary is that the objective should be clear, the means exactly proportioned to the end, and all things necessary provided before the start is made. It was an observance of these principles that made the Ashanti campaign of 1874 a brilliant success, and neglect of them that turned the French expedition to Madagascar in 1895 into a dismal failure.

The composition and strength of the force sent is an

important consideration. One of the greatest initial mistakes made by the French Government in 1895 was sending too large a force, and one in which the proportion of Europeans was unduly great. As already stated, the military foe is the one least to be feared in a tropical campaign. Locally raised troops drilled and officered by Europeans should be capable of meeting their own tribesmen, undrilled, unofficered and poorly armed. A stiffening of Europeans is necessary, but no more.[1] General Gallieni, in his "Deux Campagnes au Soudan Français," lays down this principle very clearly. "Taking into account," he says, "the unhealthiness of the climate and the peculiarly trying conditions that characterise our campaigns in the Soudan, I did not wish to encumber myself with too large a force of Europeans. I contented myself with a strong company of Marine infantry. In action this body would be available as a reserve to the native troops. The right class of soldier for the Soudan is the Senegalese rifleman." The following are the numbers respectively of Europeans and natives employed in some of our campaigns:—

Abyssinia: 3,655 Europeans, 9,833 natives.

Ashanti (first expedition, 1868), 2,284 Europeans, 2,377 black troops.

Ashanti (second expedition, 1874), 1,050 Europeans, 2,050 black troops.

It will be noted that the disastrous expedition to Ashanti in 1868 was the only one in which there was an equality in numbers between the two classes. In Madagascar in 1895 the French had 11,000 European soldiers to 3,800 natives (two battalions Algerians, one battalion Malagasys, and one battalion Haussas). It may be definitely laid down that those branches of the service not immediately concerned in fighting should be composed entirely of natives, sufficient Europeans only being sent to superintend their work. This rule applies to the Engineers, the Supply and Transport Departments, and to

[1] Reynaud, p. 247.

a great extent to the Medical Department. Men actually engaged in nursing the sick should be European, but cooks (with a few picked exceptions) and the general duty section generally should be replaced by natives. The composition of the force should be such that no fatigues and no unnecessary non-combatant duties should be imposed on Europeans.

The next point to be thought of is the selection of the men to form the force. These should be carefully picked. All men with a previous history of severe or repeated malaria should be excluded. As regards rank and file this rule should be absolute. No man whose blood contains, or probably contains, the malarial plasmodium should be allowed to proceed on the expedition, partly in his own interests, but mainly in those of his comrades. As regards officers, and more particularly those who possess special qualifications for employment, some latitude must be given. In such cases three or four examinations of the blood should be made, and if plasmodia are not found, a previous malarial history need not exclude. If plasmodia are present, the value of the individual as a strategical or tactical factor must be balanced against his potential danger as a disease centre. With an educated man occupying a tent by himself the danger is obviously not so great as in the case of a comparatively uneducated private soldier sleeping amongst his comrades. No young soldiers should be sent. Not because there is, as far as we know, any special predisposition connected with immaturity, but because the less tough a man is, the less able he is to withstand the hardships and privations inseparable from tropical active service, and, therefore, the more likely he is to break down, and consequently lower his powers of resistance. It must be remembered that in a malarious country a man with plasmodia in his blood is just as much a focus of infection as a small-pox patient would be in a barrack-room at home. Any man, therefore, who is likely to break down is a danger to his comrades, and should be rejected. The made soldier

has the further advantage that he is more knowledgeable as regards the thousand details that make the difference between comfort and discomfort, which in the tropics is almost equivalent to saying health and disease, than the raw youngster. He knows better how to cook, how to rig up rough shelters, what to eat, drink and avoid, and, generally, how to look after himself. Syphilis, of course, should exclude, unless the man has special qualifications. Some German authorities advise that all men should be tested prior to embarkation, to see whether or no they possess any special intolerance to quinine—a somewhat meticulous precaution. An absolute intolerance of this drug is rare, as a matter of experience.

The next point to be considered is the selection of the theatre of war, and more especially the selection of the line of advance into the enemy's country, and the base of operations. As regards the first named, naturally not much can be done by the sanitarian. If the enemy inhabits a malarial country, or the key of the strategical situation lies in a district where these diseases are rife, the fact has to be faced, and made the best of when the time comes. Still the Walcheren expedition is a notable instance where the epidemiological factor should have been given decisive weight. As regards the line of advance and the base of operations the case is different. One of the most important duties devolving on the Intelligence Department, in connection more particularly with campaigns in the tropics, is the collection of information as regards the health conditions of the country to be invaded. If a preliminary reconnaissance is feasible, a specially-selected medical officer should be entrusted with this duty. He should, if possible, be acquainted with the country already, but most certainly he should have a thorough practical knowledge of malarial prevention and of tropical entomology. He should ascertain the spots most suitable for landing troops, and note the possibilities in the way of systematic malarial prevention possessed by each. He should, at the same time, draw up schemes for

such work, giving an estimate of time necessary for its completion, number of labourers required, and so on. His reconnaissance should be pushed as far up the line of projected advance as possible, and he should select sites for camps and posts, temporary and permanent. It must be kept in mind that dealing, as we mostly are in these cases, with a savage foe, the first energies of the force should be directed, not against the human enemy, but against the far more intractable and deadly insect foe. Superiority in armament should be more than sufficient to cancel the tactical defects, if any, of a position selected primarily on sanitary grounds. More especially would this be the case if the base were situated (as in the majority of cases it will be) on a coast where the fire from the fleet can be made available to assist the force on land. It would be a mistake, however, to suppose that there need necessarily be any conflict between the sanitary authorities and the staff of the expedition. A good military site is in most cases elevated, and free from immediate obstruction to the view. The post, therefore, will be more or less elevated, and the surrounding jungle must be burnt or cut down, both important points as regards the prevention of malaria. Proximity to drinking-water must not be allowed to overrule the importance of avoiding as far as possible all mosquito-breeding grounds.

The place being fixed, the next point in order of importance is time. Here greater latitude is allowable, and it must be at once stated emphatically that only the most urgent political necessity, the rescue, for instance, of a beleaguered garrison, can justify the despatch of a European force to a malarious country at the unhealthy season of the year. As an instance of this may be noted the Ashanti war of 1873. The first inroad of the Ashantis took place in May of that year, but though the various threatened posts were reinforced as necessary, the first actual blow was not struck by Sir Garnet Wolseley till the 14th October. The Ashanti expedition of

1864 shows equally well the results of neglecting this obvious precaution. It is necessary to insist on this point. The expedition in 1864 was undertaken in the unhealthy season—March to June—and the mortality and sickness, almost entirely from malaria, were so great that the entire enterprise collapsed. Malaria was just as rife on the coast in 1873 as in 1864; the mortality among the Marines in the interval between May and October of the former year, whilst Wolseley was waiting for the auspicious moment to strike his blow, was 17·30 per 100, greater, as Sir Anthony Home (A.M.D. Report, 1873, p. 225) points out, than that of the army in Walcheren: whilst that of the expeditionary men after October was only 3·14%. It must be remembered that scientific prevention of malaria, as we now understand the term, was as little known in 1873 as it had been nine years earlier. The striking difference in the incidence of disease in the two expeditions was due, in great part, to the careful selection of the season in which active operations were carried out.

The question of housing of troops is most important. For this purpose portable mosquito-proof houses should be provided. These can be constructed of any material, but probably some of the modern fire-proof compositions (Eternit, etc.) would be found the most convenient. These should be provided in sufficient numbers to house all European details left at the base throughout the progress of active operations, with a margin of 10 to 20%. This is essential. No European should be made to sleep exposed to the attacks of mosquitos for a single night, where this risk can by any possible means be avoided. If the force is kept, as regards Europeans, within reasonable limits, there should be no difficulty in this respect. Any barracks of this nature sent out should be shipped on the transports carrying the troops, and it ought to be laid down that no men should be landed in advance of their barracks. If the line of advance is easy and well known, and if transport is easily procurable, then huts for Europeans should be sent

forward to the first camps on the Lines of Communication, especially those likely to be occupied permanently, as in Ashanti in 1873-1874. If the correct view of the class of campaign be clearly recognised, which is, that the contest is one not so much with man as with disease, and that the defensive measures necessary should be directed not against the attack of a human foe, but against those of tropical insects, it will be realised that the provision of adequate shelter against these pests is as essential as the provision of adequate shelter against the fire of a civilised enemy in a European war. The best type of hut or barrack is one raised on piles from the ground, much like the pattern of bungalow used in Burma.

Rations.—The scale of rations should be liberal. The meat would perforce need to be in most cases preserved, and probably biscuit would have to take the place of bread. In this case a liberal supply of antiscorbutics in the form of lime-juice, pickles, jam or preserved fruit should be supplied. A special issue of coffee, cocoa or tea should be arranged to be served out to men on night duty. It is unnecessary to say that alcoholic stimulants, in excess of the ordinary service occasional issue, are not demanded.

Clothing.—The ordinary field service kit would be sufficient, with the addition of gloves and veils for use after dark. The Japanese mosquito-bonnet might be adopted.

Special equipment.—For hospitals mosquito-netting should be issued, and the same applies to the details left at the base. It is doubtful in how far it is any use issuing squares of curtain to men actually in the field, since the use of it in camp life is difficult. It would at least be a harmless precaution. Some form of mechanically operated fan should be supplied for use in hospitals and offices at the base. The motive power can be supplied either by electricity, or by a small gas or hot air engine. Such fans not only aid in ventilation, but they also assist in keeping off mosquitos.

Instruction of men.—All officers and men taking part in

the expedition should be carefully instructed in the causation and pathology of malarial fevers, as well as of other tropical diseases. The important part played by insects should be pointed out, and special emphasis should be laid on the fact that the causation of these diseases is not of an esoteric nature, but the mere commonplace result of the bite of a commonplace insect. The miasmatical theory dies hard, and it is not easy to get the lay intellect to realise that disease is not caused by some mysterious power of induction possessed by some intangible emanation. The period occupied or spent on board ship can profitably be utilised for this instruction.

We now come to the period of active operations, and the first point to be considered is the base of operations.

Amongst the instructions to Sir Garnet Wolseley in 1873 it was laid down that if the use of European troops became imperative everything should be prepared in anticipation, and not a man be landed before the moment for advance into the interior arrived. The accommodation for troops at the base need not therefore be large, and may be limited to that for the details left there on duty, with a small margin.

Measures directed against the breeding-grounds of the mosquito, which should have been, as already stated, drawn up by the special medical officer entrusted with the preliminary survey of the base, must be carried out as in cantonments in time of peace. A specially selected medical officer should be placed in charge of these operations with *executive* and *disciplinary powers*, a step that has been proved to be so effective by the officers of the United States Army in Cuba and elsewhere. This officer should be provided with an ample supply of native labour, and such number of European subordinates, preferably N.C.O.s of the Royal Army Medical Corps, as the size of the locality demands. Any parsimony in this direction may have serious results. Presumably, the base will not be in the immediate vicinity of a native village. Nothing but the most urgent strategical necessity can justify such a

selection. In addition, all native establishments must be encamped or hutted at a distance from the European lines. Their defence and discipline must be ensured by appropriately situated outposts, electric lights and barbed wire fencing being used to assist in these and prevent desertion. The guns of the fleet can, of course, assist in defence. It may be necessary to erect a base hospital, but patients should be retained there only so long as to permit of rest prior to embarkation. Two hospital ships should be used on any tropical expedition. Of these one would always be available for the reception of sick coming from the front, whilst the other was carrying its passengers to the nearest healthy port or island.

It is essential to insist that there is no difference between the conditions at the base, and the conditions of any cantonment during ordinary peace time. Everything that is possible in the latter case is possible in the former, and should be carried out with the same thoroughness and the same careful organisation. The subject of prevention of malaria in cantonments is treated of elsewhere, so more need not be said here. The important point, and this cannot be too often repeated, is that undivided responsibility should be given to one man, and that authority of equal extent should go hand in hand with the responsibility. So only can success be hoped for in the war against malaria.

Leaving the base of operations, the force now proceeds on its mission. Detachments will no doubt be made from time to time of small bodies to hold fortified posts on the Lines of Communication. These detachments may have to remain stationary for the greater part of the campaign, and in their case regular anti-mosquito measures must be undertaken, as far as possible on the same lines as at the base, or in cantonments in peace time. The proximity of a native village will necessarily be avoided as far as possible. Where the tactical situation necessitates such proximity, the village, if deserted, must be destroyed by fire directly the permanent occupation

of the post is decided on. If still inhabited some trouble may arise, but it would be preferable to forego some minor tactical advantage than to run the great risk of disease that such a situation entails. If the tactical considerations are paramount, then the inhabitants should be, if possible, evicted, fresh shelters being built for them at a little distance, and any necessary compensation paid. The site could then be, as already recommended, cleansed by fire. Since it may be accepted that the mosquitos already existent in the neighbourhood of the post are infected, it would be advisable to adopt the method originally suggested by Nuttall (*Journal of Hygiene*, 1902, vol. ii. p. 73) of constructing mosquito-traps from small boxes, lined inside with some dark material.

Dr Blin, Principal Medical Officer of Conakry, utilised a similar method in Dahomey in 1905 (*Archiv. fur Schiffs—und Tropen Hygiene*, Bd. xiii., 1909, *p.* 645). He noticed there that the adult insects were in the habit of sheltering themselves during the hot hours of the day in the burrows of the land crab, and in holes between the roots of mangroves. He accordingly caused small holes to be dug in the ground about 16 inches deep, at a somewhat acute angle to the surface. These holes were so situated as to be protected from direct sunlight, and were, if properly placed, invariably crowded with mosquitos. These insects remained in their shelters until 4 P.M. as a rule, and accordingly between 2 and 3 P.M. on each day the holes were burnt out by means of a small torch, consisting of a stick about 5 feet long, at the end of which a piece of rag or tow, soaked in petroleum, was fixed. Such a torch will burn for about ten minutes, and one will suffice to burn out fifteen to eighteen holes if in close proximity to each other. A well-trained native can attend in this manner to one hundred mosquito-traps in a day. The cost is trifling, 3 litres of oil are sufficient for five hundred torches. Fresh holes must be dug once a fortnight, as the old ones become saturated with oil. As far as possible all permanent posts

should be provided with mosquito-proof huts, and in their absence mosquito-netting should be supplied to the men.

Night duties should be reduced to a minimum, and sentries relieved every hour, or even oftener. The very fullest use should be made of mechanical appliances, barbed wire entanglements, searchlights, etc., with a view to lessening the strain on the sentries. Hot coffee, cocoa, or tea should be freely available for all men exposed to the night air. On no account should fatigues, which can be performed by natives, and for which natives are available, be carried out by Europeans. The question of the prophylactic use of quinine will be discussed later.

With regard to moving bodies of troops, which do not occupy any one camp for more than forty-eight to seventy-two hours, it is obvious that measures directed against the breeding-grounds of the larvae can be of no avail. Before any such measures can have effect the force will already be many marches away. The defence therefore limits itself to the protection of the men from the adult mosquito, and to preventing the access of the latter to the exposed surfaces of the body, or, as a last resort, to rendering individuals immune to the plasmodium by the use of quinine.

The former can be achieved by the use of mosquito-veils and gauntlet gloves by men on night duty, and, doubtfully, by the use of mosquito-netting when asleep. Unless a man is provided with some sort of framework whereon to support the curtain, it is doubtful in how far this precaution is of use. Obviously a complete set of poles for mosquito-curtains cannot be carried by each man. The issue of a couple of yards of netting to each member of the force is at the same time a precaution that may with advantage be adopted. The use of essential oils for the purpose of anointing the exposed surfaces of the body has much to recommend it. It causes less discomfort than that entailed by the obstruction to respiration when veils are worn. There is at the same time less hindrance

to the vision, and the hands are less hampered in the manipulation of the fire-arm than is the case with gauntlet gloves—important points in the case of sentries. Kerosene or paraffin oil, everywhere procurable in the tropics, serves this purpose as efficiently as any of the more elegant aromatic oils.

Last of all we come to the prophylactic use of quinine. When it is impossible to control the breeding of mosquitos, or to protect oneself from the bites of the adult mosquito, then it is necessary to have recourse to the prophylactic administration of quinine. But it must be premised that such a system of prevention of malaria is a confession of failure. Success may be difficult, or even impossible, under the many vicissitudes of campaigning in tropical jungles when troops are constantly on the move. Success may be difficult, but it should not be impossible, under conditions such as prevail at a properly selected and administered base of operations. The prophylactic administration of quinine should not therefore be looked upon as anything but a last resort, or a merely temporary expedient in the case of troops living under settled or comparatively settled conditions. The prevention of disease by the administration of drugs is as unscientific as the treatment of disease by relief of symptoms only. Both courses may, under certain conditions, be imposed on us by the *force majeure* of circumstances, but neither should be deliberately chosen as the chief method of defence, either in sanitation or therapeutics. The administration of quinine has this great drawback, that each individual has to be protected separately. The total mass of disease in the country is in no way affected. Every newcomer is still exposed to the risk of infection, just as much as his predecessors, and each such person must be protected. On any scientific system of sanitation the disease should be attacked, not after it has infected the patient, but before. Measures directed to the latter end protect the individual, not the community, whilst true sanitation aims at protecting the individual as an item merely in the community. If quinine

is administered, I lean personally to the German method of giving two large doses on two consecutive days, 10 to 15 grains, according to the idiosyncrasy of the individual. As a matter of practical administration this should be done once a week, on Mondays and Tuesdays, or any other days, but the same days of the week should be adhered to throughout. The dose is less likely to be omitted in such a case.

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61. The Prevention of Malaria among Troops under Peace Conditions. — The vast importance of malarial fever can be appreciated by a glance at the following table, from which it will be seen that out of a total strength of European troops serving at home and abroad of 215,467 there are admitted to hospital each year 19,092 patients suffering from this affection. The average constantly sick reaches the large total of 653·03, which means that a number equal to the ordinary strength of a regiment is always *hors de combat* owing to this disease.

EUROPEAN TROOPS AT HOME AND ABROAD.

AVERAGE STRENGTH, 215,467.

Malaria.

	Admissions to Hospital.	Deaths.	Invalids.	Average Constantly sick.
1908. Gross . . .	19,092	47	197	653·03
1908. Ratio per 1,000 .	88·6	0·21	1·87	3·07
1903-1907. Ratio per 1,000	64·4	0·13	2·39	2·47

The next table shows the incidence in some of the countries where malaria is most rife, and a comparison is here given of the effects of the disease during 1908 and the five previous years.

MALARIAL INCIDENCE.

Place.	1908. Average strength.	1908. Admissions into Hospital.	1908. Deaths.	1908. Invalids.	1908. Average No. constantly sick.	1908. Ratio per 1,000. Admissions.	1908. Ratio per 1,000. Deaths.	1908. Ratio per 1,000. Invalids.	1908. Ratio per 1,000. Constantly sick.	1903-07. Average ratio per 1,000. Admissions.	1903-07. Average ratio per 1,000. Deaths.	1903-07. Average ratio per 1,000. Invalids.	1903-07. Average ratio per 1,000. Constantly sick.
Crete	640	33	...	...	1·64	51·6	...	...	2·56	229·3	0·63	...	8·26
Cyprus	104	9	...	...	0·33	86·5	...	...	3·17	99·8	3·84	...	4·18
Jamaica	444	37	...	...	1·44	83·3	...	...	3·24	79·8	0·37	0·37	3·27
Western Africa	258	234	3	6	6·39	907	11·63	23·26	24·77	721	5·23	36·62	24·46
Mauritius	863	258	7	106	18·69	299	8·11	122·83	21·66	263·7	1·59	24·5	12·56
Ceylon	303	29	...	...	0·91	95·7	...	...	3	28	...	0·9	1·23
South China	2,012	515	...	17	20·42	256	...	8·45	10·15	348·8	1·14	6·14	13·65
Straits Settlements	1,237	236	...	5	8·36	190·8	...	4·04	6·76	388·2	1·92	11·98	13·43
India	68,522	16,763	35	62	572·17	244·6	0·51	0·9	8·35	172·2	0·3	2·96	6·48

During 1908 malaria gave the largest gross number of admissions and therefore the highest admission rate per 1,000 of any disease on the nomenclature. This fact alone must point out the transcendent importance to the military sanitarian of finding some means of lessening the disease and saving the enormous waste entailed by the loss of service of our soldiers and sacrifice of public money through inefficiency.

On looking at the table it will be seen that Western Africa heads the list under nearly every column of ratios per 1,000. The West Coast has always held a bad record for malaria, and this disease can be said to cause from 50% to 60% of the total admissions, invaliding and average constantly sick for all diseases at the stations in this command. The West Indian troops suffer almost as severely as the European, but the West African regiments are much less susceptible.

The country which costs us most dearly is India, where we can see how terribly heavy the toll of sickness caused by malarial fever is. The figure for 1907 was a great improvement on that for the ten years previous, but, unfortunately, 1908 was a wet year, fever was prevalent amongst the native population, and, in consequence, the figure again rose, in spite of all efforts to diminish it.

INDIA.

APPROXIMATE STRENGTH OF BRITISH TROOPS, 70,000.

Malarial Incidence.

Year.	Admissions.	Deaths.	Invalids.
1898. .	28,382	50	485
1899. .	16,579	36	246
1900. .	19,445	62	190
1901. .	18,217	35	418
1902. .	15,367	45	245
1903. .	17,037	35	244
1904. .	12,112	23	259
1905. .	7,947	10	126
1906. .	12,601	22	136
1907. .	10,662	15	274
1908. .	16,763	35	62

It should be noted that since 1903 a large number of men, averaging from 4,000 to 5,000 each year, are treated in barracks.

Amongst the native army of India malaria is also very prevalent. During the last ten years the average strength of these troops has been 124,523, of whom 34,074 have been each year admitted to hospital, to say nothing of those treated in barracks and not coming on the returns.

Having thus briefly outlined the incidence of malaria at some of the various stations of British troops, there is now to be considered what means of prevention can best be employed to counteract such serious losses. In the first place, it must be presumed, when formulating any means of prevention, that readers are believers in the part played by the mosquito in the transference, spread and maintenance of the malarial parasite, and have absolute faith in the formula of "No mosquitos, no malaria." It would appear mere waste of words to make such a statement at the present day, were it not that experience has shown how even now there are people, including members of the medical profession, who still retain doubts of the mosquito being the sole carrier of the malarial parasite. All recommendations here made will be based entirely on the proved facts of malarial transference by the aid of certain varieties of Anophelines.

In formulating the measures of prevention most applicable to troops, it must always be remembered that the soldier cannot be thought of in his position as a unit only; there also has to be taken into consideration the inhabitant of the country in which the soldier is residing. It may be stated, as a general axiom, that if the native suffers from a certain disease, it will almost necessarily follow that, unless extraordinary precautions are taken, the soldier will inevitably suffer from a like complaint. The soldier is largely dependent for his health on that of the community around him.

Therefore it comes to this in many cases, that to check malaria amongst the troops it will be necessary at the same

time to lessen it amongst the surrounding inhabitants. Thus the question can be seen to be a much wider one than would at first appear.

Preventive measures may be divided into :—

1. General measures of prevention.

2. Personal measures of prevention.

1. The general measures of prevention are such as will be undertaken by the State, Local Government, or Community at large. As a rule they involve a considerable expenditure of capital, as well as a large amount of thought, trouble and probable legislation by those in power. For these reasons obvious means of cutting short malarial incidence have been on more than one occasion allowed to lapse, or only very inadequately provided for by Local Government officials. Failure must necessarily follow; and though the authorities may endeavour to make a scape-goat of the means recommended, the blame lies entirely at their own door.

In other instances financial considerations alone must block the way to improvement; and in certain of our colonies it is a question for serious debate whether the necessary funds should not be provided temporarily by the Imperial Exchequer. It is heart-breaking to look on the results of malarial havoc, knowing at the same time that this could be controlled, were power and money employed to the best advantage; and yet neither can be obtained.

These general measures can be dealt with but briefly in this section, but reference must be made to them as affecting the population surrounding the troops, on whose health the latter are so largely dependent.

At the present time there are two schools of opinion. One relies mainly on the destruction of the larval mosquito; the other, believing that such a desideratum is without the range of possibility, affirm that quinine dosage forms the mainstay of prevention.

The first school, relying chiefly on larval destruction, looks

upon quinine as a valuable aid certainly, but not to be placed upon the same footing as the destruction of the primary agent, except in certain cases where larval destruction is almost hopeless. The second school base their preventive measures primarily on quinine dosage, and hold that if the malarial parasite is prevented from remaining in the human blood-stream by the administration of quinine, then it is impossible for the mosquito to become infected.

In every country where malaria is present there is some diversity of opinion on which of these methods chief reliance should be placed. This matter will be dealt with fully in other sections, so that no further reference will be made to it here, save to mention, as a general statement, that in the majority of our possessions larval destruction is the one of primary importance and trial.

A. The *General Measures* may therefore be cited as follows:—

(1) Larval destruction.

(2) Segregation of the soldier.

(3) General quinine treatment.

(1). *Larval destruction*, under this heading, means the elimination of all spots where the Anophelines may find a home or a suitable breeding-place, from which it can reach barracks, cantonments or other sites where troops may be quartered. The question at once arises: What is the probable or possible flight of one of these insects? A reply of fact and not of theory it is impossible to furnish. Some years ago many authorities would have judged the probable limit of flight at a few hundred yards, but facts have arisen since then, showing that mosquitos under favourable climatic conditions can and will travel several thousands of yards from their breeding-place to their feeding-ground. To lay down a precise distance of safety is quite impossible, but a reasonable limit, within which no breeding-spot should be allowed around barracks, may be

quoted at 2,000 yards or thereabouts. Now many of our barracks abroad are built on small plots of ground belonging to the War Department, but the land closely adjoining may be quite without their jurisdiction ; the consequence being that the Military Authorities have no power to enforce the abatement of evils existing on the ground of neighbouring tenants.

At the present day no barracks should be built in the tropics or sub-tropics on ground which does not include the control of an area of 2,000 yards' radius from it as the centre of a circle. Unless this plan is carried out the troops occupying the barracks must of necessity be exposed to all dangers arising from a neglected condition of the surrounding area, as well as the risk of being infected with other diseases from which the inhabitants living on this ground may be suffering. Many examples of barracks situated in this faulty position might be quoted from our Colonial garrisons.

When the State or War Department owns or controls the land around barracks, it is incumbent upon them to place this ground in such a condition that it shall not act as a breeding-spot for mosquitos.

If such a land is not their property, and is forming a favourable source of origin for Anophelines, it is essential that the ground should be purchased, in order to gain complete control over it. The purchase may involve great expense, but this would probably soon be covered by the saving effected in the improved sick-rate of the troops.

(2). *Segregation of the soldier.*—This has been referred to under the previous heading. It may be stated fairly conclusively that the wider the separation of the soldier from the native inhabitants the better will be his health. The barracks that have been carelessly placed in the midst of native towns, or that have become encroached upon and surrounded by native houses, will show a far heavier incidence of disease than barracks standing in isolated positions. The reservoir of the malarial parasite is to be found chiefly in the children of the

native inhabitants. An infected village with a high spleen rate amongst its children will contaminate any camp or settlement within a wide distance of its site. Such a condition of things exists in numbers of our garrisons abroad.

Take the case of so many of our Indian stations. The barracks may have originally been placed at a reasonable distance from the native town, but a bazaar has been allowed to grow up alongside them for the use of servants and others serving on or trading with the troops. Many of these Sudder Bazaars contain several thousands of inhabitants. Malaria or any other disease from which they may suffer must inevitably spread to the troops.

Again, within the Indian cantonment, until quite recently the native was allowed to grow any sort of crop, and to do practically whatever he wished with the ground rented. The consequence was, and still is, in some places, that rice fields existed within quite a short distance of barracks, to say nothing of other crops requiring an abundance of moisture, which so generally means neglected ditches and the formation of small marshes. The outcry against the continuance of such a state of things has often been raised, but is invariably met by the answer that if such crops are done away with, the cantonment funds would disappear and financial ruin ensue. It is the duty of the State to insist on an abatement of such dangers; and if in any case its continuance cannot be stopped, then the troops should be moved and the station evacuated. Until the State can appreciate the enormous losses involved by the sickness of our soldiers from malaria and its *sequelae*, it is almost hopeless to expect works to be taken in hand, which must appear to the authorities to involve a large expenditure of capital without any evident financial return.

(3). *General quinine treatment.*—By this is meant quinine treatment of the general population of the country in districts around the location of troops. This lies outside the measures to be undertaken by the Military Authority, who would only

be in a position to advise and not insist on such a campaign of defence. It would mean action on the lines of the Italian school, and will be fully dealt with in another section. The military forces would benefit very greatly, as the chief source of their contamination, namely, the native children, would be brought under treatment, and their capability of spreading the parasite by acting as reservoirs materially lessened.

B. Personal Measures of prevention must now be considered. These are such as can be carried out by units themselves and will involve no great initial expense, as is so often the case with the general measures.

The various means employed may be enumerated as follows:—

(1) The attack on Anopheline larvae in their breeding spots.
(2) The defence against the adult Anopheline.
(3) The segregation of malarial patients, and their protection from the Anopheline mosquito.
(4) Segregation from the native.
(5) The administration of quinine.

(1). It is one of the first duties of an officer commanding a barrack or cantonment to see that the same is kept in a clean and sanitary condition. It must also be the duty of this officer to see that the place under his control in a malarious country is in such a state that mosquitos will be unable to find any breeding spots on its area. If he does not himself understand or appreciate the hygienic importance of this order, he must be compelled to do so by a higher authority, and be made to accept the advice of experts who have made a special study of the subject.

The attack on mosquito larvae can best be carried out by the formation of "mosquito brigades," or "working gangs," consisting of men especially trained to such work. These men may be either soldiers or natives of the country. It

is not considered that the former are suitable for these duties Soldiers are wont to despise such employment, and think it beneath their dignity to carry it out. In addition, there is always the likelihood of their being continually changed and taken for other duties, just at a time perhaps when their services are most urgently required.

It must also be borne in mind that the heaviest work will come just at the most trying season of the year, when the weather is intensely hot and perhaps saturated with moisture In a tropical country soldiers should certainly not be employed

It remains, then, to rely on the native. It has been found in practice that, if the proper class of native is selected, he is capable of being trained very efficiently to the work; but of course he will always need the supervision of Europeans. The number of these men required will vary in accordance with the size and conditions of the barracks, cantonment or War Department ground. Experience has shown that it is better to divide up the men into separate units or working gangs, rather than have a number of men engaged indiscriminately. A convenient working gang would consist of a head man with two or three underlings. The head men should be selected for their special aptitude in the work and should receive more instruction than their underlings, in order that they in their turn may be able to impart the information received. All the men employed must be given a general insight into the causation of malaria and be taught the life history of the mosquito. They must know the different species, both of the larvae and adults, together with their habits. They must also receive a course of instruction in the best means of dealing with casual water, in ditching and rough draining. These men should be under the direct control of the medical or other officer appointed in charge of malarial work, and he will be responsible for their instruction.

Different ways of working the gangs can be arranged as best suits the individual place. For instance, the station may be

divided up into six different areas, and a gang made responsible for each ; or gangs can be employed on one of these areas for a day in each week. Again a gang may be appointed to each regiment or unit, and a man, preferably a N.C.O., made responsible for its working. The general duties of these gangs will be shortly as follows :—The exploring and examining of all ground around the barracks or cantonment; the collection of all tins, pots, pans, or other rubbish capable of holding water; the cutting and removal of all waste vegetation, brushwood and scrub; the clearing and grading of ditches, the filling up of holes and hollows, the draining away of casual water and small marshes; the examination of water-tanks, water-butts, wells and cess-pools; the search for plants and holes in trees retaining water; the treating with paraffin-oil, tar or other larvicide of all water that cannot be disposed of or properly protected.

It is advisable that attention should be paid, not only to the Anopheline larvae, but also to those of *Culex* and *Stegomyia*, as the destruction of the two latter, which are usually the more domestic species and therefore more in evidence as nuisances, will show the inhabitants of the station that something is being done. Besides, in many places, these mosquitos are a veritable pest, the *Stegomyia* by day and the *Culex* by night, and their destruction is worth some outlay, if only to obtain increased comfort.

When dealing with these larvicidal operations, the question of the station or cantonment irrigation is certain to arise. Where a canal supply is in force, as in many parts of India, it is well-nigh impossible to act efficiently on the Anopheline larvae, unless such irrigation is stopped. The main canal, as it passes through the cantonment, is broken up into numberless small channels for the distribution of the water. These generally become little better than rough and neglected gutters, overgrown with vegetation, and allowing the water to stagnate. Small patches of marsh or water-logged soil are often formed

by the side of these channels from leaking or overflowing of their banks. These will form ideal breeding spots for most varieties of the Anophelines. On the other hand, it has to be recognised that if irrigation is cut off, the cantonment will be turned into a desert. Irrigation from wells is by no means so objectionable. The supply of water is limited by the labour necessary to raise it from the well, and therefore only a sufficient quantity to irrigate the crops themselves is drawn. Water seldom stands wasting in channels or sides, so that the Anophelines have little opportunity of finding any breeding pools.

A medical officer should be detailed for each station or cantonment to look after all malaria work, and he should be placed in charge of all the working gangs to direct and supervise their operations.

At the headquarters of each district or province a medical officer possessing special knowledge on the subject of malaria should be appointed. It should be his duty to travel from place to place, to supervise the work being carried on, and to advise as to future procedures. All reports on malaria from the out-stations should be submitted to him, in order that he may collect the various details, and be in a position to form a reliable opinion on the efficiency or otherwise of the different measures of prevention.

(2). *The defence against the adult Anopheline.*—The means at disposal are mosquito-proof houses and mosquito-curtains.

In Europe, or even in a sub-tropical climate it is easy to lay down rules and carry them out as regards the use of the above means. The matter resolves itself into one only of expense for their provision. In the tropics the situation is far different. Here the most prominent question is: Will the troops or others make use of such means if they be provided? or even it may be asked: Can they be allowed to do so? To those who have experienced hot weathers on the plains of India, this point is one that is only too vividly appreciated. To

expect any one to remain under a mosquito-net during the day or night-time of the hot weather, or even to remain in a room, the windows of which are blocked by wire-gauze netting, is scarcely reasonable. Existence would be unbearable, and in all probability sacrificed to an attack of heat apoplexy. Unfortunately the use of mosquito-curtain and punka is incompatible, at least for the soldier. Mosquito-proof rooms and powerful electric punkas or fans might be borne. In many parts of India, for five or even six months of the year, punkas are a necessity of existence, and therefore curtains cannot be made use of. It is impossible to lay down definite rules in regard to this question. In each district or station the various points will have to be taken into consideration in a common-sense manner, and corresponding recommendations made. Mosquito-curtains should undoubtedly be provided for all soldiers in a malarious locality, and they should be made to use them if the climatic conditions permit. The form of curtain varies. The bell pattern is the cheaper, more easily managed and hung, but does not allow the same air-space within it. This appears a minor point in the sub-tropics, but is soon appreciated under tropical conditions. The old pattern square shape is now again being used on this account, but the obstruction to ventilation and air movement becomes very great when each bed in a barrack room is fitted with these curtains. Men must always be instructed in the best method of hanging them, otherwise they are worse than none at all, and act simply as traps. In many cases it has been found that although curtains are provided, the men are not in the habit of using them when on guard or attached duty, and consequently become infected. The provision of wire-gauze for all doors, windows and ventilators in a barrack-room is one of theoretical excellence, but one of great practical difficulty. The expense, both initial and recurring, is of some magnitude, and constant watch must be kept on the proper fitting and closing of the openings; otherwise the soldier is

sure to remove what he will consider are useless obstructions to the breeze.

In certain isolated and dangerous positions, where a small number of men are on duty, the employment of this method is undoubtedly a sound one, but as a general preventive measure it has not been taken up for our garrisons.

As regards the destruction of the adult mosquito but little can be done. The majority of the Anophelines are night feeders, and as a general rule will leave a room before daylight. There are some varieties, however, which will remain a certain time after engorgement, and can be found in the dark corners of rooms. Various means have been devised for killing off these insects by fumigation, such as the burning of sulphur, formalin or other chemicals. These methods are of but little use in the tropics, where it is most difficult to render a room in any way fume-tight. The safest procedure consists of the regular sweeping out of all dark corners, removal of unnecessary hangings, and thorough cleanliness. Many so-called specifics have been brought forward, purporting to ward off the attacks of mosquitos when smeared on the skin, such as soaps, ointments, oils, etc. Some of these are excellent, whilst their virtue lasts, but the latter is very evanescent, and few of them are found to be efficient for more than about half an hour.

(3). *The segregation of malarial patients and their protection from Anophelines.*—This is a most important matter and one that has been much neglected up till very recently. It was customary to place patients suffering from malaria in any ward with other patients not yet infected, no provision being made or even thought of to prevent infection being carried from one to the other. In consequence, many men who were in hospital for some trivial complaint became infected with the parasite during their detention. At the present day it is understood that a patient suffering from malaria is a danger to his non-infected neighbour, and he should, therefore,

be placed in a separate ward, and provided with curtains, or if these cannot be borne, then the ward must be rendered impervious to the entrance of the mosquito. In any station where malaria is prevalent, these precautions are essential. A ward containing malarial patients unprotected from the attacks of the Anopheline carrier, is a grave potential danger to the surrounding community, and should not for one moment be tolerated.

(4). *Segregation from the native.*—The importance of this has been referred to previously under general measures of prevention, when insistence was laid on the advantages to be derived in separating soldiers entirely from the native houses and population around. It must not be forgotten that the soldier may still easily contract the disease by visiting places or towns, where malaria is rife amongst the inhabitants, and it is very necessary to do everything in one's power to prevent such excursions, more especially after dark.

When troops attend camps of exercise or are on the march through a malarious country, the encampments should always be placed as far as possible from any native village or from any spots where infected mosquitos are likely to be prevalent. The same advice can be given as regards shooting-trips, on which the officer often, and the soldier occasionally, proceed. Native guides will always try to induce a party to pitch their encampment in close proximity to a village or water of some kind, for obvious reasons of supply. This should not be allowed, but the very opposite plan adopted. The use of nets for soldiers when on the march is almost impossible, crowded as they are together in small tents, but for the officer it is quite a simple matter to carry a small bell-shaped net and sling it up from the tent wall or branch of a tree at night. On shooting-trips during the malarious season a net should invariably be carried. It is very light and easily packed, if the supporting ring is hinged to fold over.

(5). *The administration of quinine.* — This drug must be

employed for the soldier as for the general population, namely to (*a*) cut short the actual attack, (*b*) prevent relapses, (*c*) prevent the infected acting as reservoirs and potential spreaders of the parasite, (*d*) act as a prophylactic agent.

(*a*) The various methods of administering quinine as a curative agent will be dealt with in other sections, so that further reference to this point will not be made. The treatment of the soldier differs in no way from that of a man in civil life, and army medical officers have now a free choice of any method which they may select.

(*b*) It may be taken for granted that, if a patient has been infected with the malarial parasite and suffered from an attack of fever thereby, it will require a period of probably three, certainly two months continuous and rigid treatment, before the parasite is rendered more or less inactive. To say that it is destroyed is not possible, however energetic the treatment may be.

The failures of quinine to prevent so-called relapses are not generally failures in the true sense, as the recrudescence of the attacks is in the large majority of such cases due to a re-infection. This is the reason why it is so extremely difficult to cure a patient when he still continues to reside in a malarious locality. Within the last few years the importance of the after-treatment of all soldiers affected with malaria has been appreciated by army medical officers, and nowadays every care is taken to carry on continuous treatment over a period of several months and to record results.

It may be presumed that the blood of every suspicious case of fever now admitted to a military hospital will be examined for the malarial parasite,

and that the diagnosis of malaria will never be made unless a positive result is found. When a malarial patient is discharged from hospital, some such scheme as the following may be employed. The man is given a paper, telling him on what days he must attend for doses of quinine. A duplicate of this order is kept by one of the hospital staff, whose duty it is to give the paper, note the dates of attendances, and report any failure to do so.

The *raison d'être* of continued treatment should always be briefly and reasonably explained to the patient, in order that he may grasp the importance of taking quinine regularly, not only for his own sake but in the interest of his comrades. The treatment must be enforced; but as a general rule there is not much difficulty in making a soldier who has once had an attack of malaria realise that it will be to his benefit to continue treatment. Every facility must be given to him for obtaining the necessary doses.

There is some variance of opinion as to the best method of giving the drug. For soldiers the most suitable plan is perhaps the administration of 10 or 15 grains of the sulphate or its equivalent on two successive days every week or on three successive days every ten days over a period of three months. During this time the blood should occasionally be examined for the presence of parasites and the treatment modified if necessary.

(*c*) To prevent the infected acting as reservoirs of the parasite: Treatment for this object is in reality bound up with that mentioned for the prevention of relapses. If the latter do not occur, it is improbable that many or in fact any gametes are

circulating in the patient's blood. Similar lines of giving quinine can therefore be recommended.

(*d*) As a prophylactic agent: On the use of quinine for this object there is a considerable diversity of opinion, and the latest military reports are not by any means favourable to any definite conclusions of the benefit ensuing from its administration. (A.M.D. Report, 1908, India.)

However, there has been undoubted evidence of beneficial action exerted by its use, and it is therefore the duty of the medical officer in charge of troops to administer the drug whenever malarial conditions are widely prevalent. It must always be remembered that positive results, as the outcome of quinine in prophylactic dosage, are most difficult to estimate. Negative results are declared only too vividly; and a few of the latter as facts far outweigh any estimations of the former as theories. In considering the results, it should be also brought to mind that a large proportion of the cases admitted to hospital, probably more than 50% in some stations, are relapses, and that the administration of quinine may greatly lessen the number of such relapses, quite apart from any action in preventing fresh infections.

The difficulties encountered in carrying out an efficient prophylactic issue are very great. The soldier is extremely suspicious, when forced by order to undergo such a dosage, and will often do everything in his power to evade it. Considerable tact must always be employed in order to win over his confidence. In any case the drug must be taken by order, and in the presence of the medical officer or other reliable person. An issue to volunteers only soon resolves itself into a farce.

The drug may be given in solution, or in the

> form of pills or tabloids. Solution has the advantage that it is always at hand and easily made up, but the taste is of course extremely disagreeable to most people. Pills are convenient, but take some time to make, and do not keep well under conditions of heat or moisture. Tabloids are really the most useful form, and given as a preventative only, they are unlikely to pass through the intestine unchanged. The uncoated form should be used. As regards this, there is a note in the A.M.D. Report, 1908, to the following effect. "As is usual the men disliked the quinine parades, and many adopted every means they could to avoid swallowing the dose; tabloids of quinine were found unsuitable, since many men kept them in their mouths until the parade was dismissed, and then threw them away. Hence, quinine in solution was administered, and each man had to shout out his regimental number immediately afterwards to prove that the dose had been swallowed

The above is one typical instance of resentment at compulsory treatment, and points out how extremely difficult it must be to rely entirely on the results of quinine prophylaxis under military conditions.

As regards the dose, there is some difference of opinion. A daily administration of 5 grains cannot be recommended for soldiers, owing to the trouble it involves in the parading, etc., of the men. In India during 1908, where in 53 out of 58 plains stations quinine was given as a prophylactic during the malarious season, in the majority of the cases 10 to 15 grains were used on two consecutive days in each week. This seems to be as good a method as can be recommended.

One most important point not yet referred to is the instruction of the soldier and others in the causation and spread of malaria.

All such instruction should be given in a way which will appeal to the imagination, and the language used should be couched in the most popular style. Short descriptions of lantern slides are the very best means of imparting elementary knowledge of this kind. Each medical officer appointed to a division or district as an expert on malaria should be provided with a set of lantern slides and suitable lantern, and give short demonstrations in each station throughout his district. A full but elementary understanding of the cause of malaria and the *rôle* of the mosquito in its spread, will explain to the soldier the reason of many precautions which he may have failed to understand and thereby neglected.

It will be found that the men are ready enough to receive such instruction, and become greatly interested in the subject, provided that the facts are placed in a simple and straightforward manner before them.

I. PORT LOUIS, MAURITIUS, BARRACKS AND HUTS.

The latter had to be evacuated owing to the prevalence of malaria amongst their occupants.

2. THE CAUSE OF THE MALARIA AMONGST THE TROOPS IN THE HUTS.

This stream was situated at a distance of 400 yards, and formed a most favourable breeding-ground for *P. costalis*.

3. ANOPHELINE BREEDING-GROUND IN A COLONIAL CANTONMENT (MAURITIUS).

4. PART OF AN OLD FRENCH FORTIFICATION AND MOAT NEAR PORT LOUIS, MAURITIUS.

P. costalis larvae were swarming amongst the grass. Malaria very prevalent in the barracks situated about 900 yards away.

COOLIES "ROUGH TRAINING" A STREAM.
(The Latanier, near Port Louis, Mauritius—the river of Paul and Virginia.—R. Ross.)

5. THE SAME FINISHED.
This stood intact after several floods.

6. INDIAN VILLAGE IN THE MIDDLE OF A MARSH (CLAIRFOND, MAURITIUS).

Spleen rate of children 96%. Barracks (Native) 400 yards distant. Barracks (European) 1,000 yards distant. Malaria very prevalent amongst the troops.

7. A NATIVE HUT IN THE SAME VILLAGE.

Showing how it was surrounded by marsh. Anopheline larvae swarmed here.

8. CLAIRFOND MARSH IN MAURITIUS, WITH VILLAGE NEAR BY.

Malaria rife amongst troops in barracks situated at several hundred yards' distance.

ADDENDA

62. Suggested Terminology for the Phenomena of Cytogenesis.—Several such terminologies have been proposed, among others by myself. That of F. Schaudinn is now extensively used, but has the defect of attempting to limit the word *spore* to the first generation of daughter cells proceeding from the zygote, and the word *schist* only to subsequent generations. His merozoites are in fact spores, his spores merozoites: his spores are really produced by his schizogony and his schizonts by his sporogony. I think therefore that a more general system, in which each root is invariably used in a strictly defined sense, may still be proposed for consideration, briefly, as follows:—

Schist = a division of the centrosome and nucleus only.

Clast = a division of centrosome, nucleus and cytoplasm only.

Mere = a division of the whole cell.

Spore = a mere when more than two meres are produced at a time.

Protoschists or *protoclasts* are the first schists or clasts produced in the dividing cell, and *protomeres* or *protospores* the first meres or spores (starting with the zygote).

Deutero- and *trito-*, etc., for succeeding generations.

Gametes = the sexual elements. *Microgamete* = sperm, *macrogamete* = oön.

Zygosis = conjugation, and *zygote* = the resulting body.

—*geny* = production *by*—*e.g.*, parthenogeny, sporogeny.

—*genesis* = production *of*—*e.g.*, spermatogenesis.

—*plasis* = making of—*e.g.*, gametoplasis.

—*phase* = phase or cycle—*e.g.*, gamophase (sexual cycle), agamophase (asexual cycle), schistophase, merophase, zygophase, etc.

—*cyte* = a cell destined to produce—*e.g.*, gametocyte.

—*phore* = a body actually producing—*e.g.*, sporophore (sporulating body), spermatophore (flagellate body).

—*cyst* = a capsule containing—*e.g.*, oöcyst, clastocyst.

—*id* = a body connected with, or belonging to, the phase of—*e.g.*, merid, sporid, gametid. Also haematid (red corpuscle), leukid (white corpuscle), micrid (blood plate), etc.

Descriptions should, I think, commence with the definite starting-point of the zygote. Thus I should describe the general cytogenesis (or biogenesis) of *Plasmodium* briefly as follows :—

Zygosis occurs in the stomach of the mosquito. The zygote penetrates into the stomach wall; and after a short schistophase produces about eight to twelve protoclasts which, continuing schistogenesis, produce a large number of deuteroclasts. These, escaping from the cyst, become protospores; which pass by route of the insect's salivary glands into the vertebrate blood, where each penetrates a haematid. Here the sporid, after another schistophase, produces six to thirty deuterospores, the sporophase being thus continued indefinitely. The gamophase commences (by stages not yet clearly seen) in the vertebrate blood. The oöcyst has one oön, and the spermatocyte, say, four to six sperms; but gametophoresis and zygosis do not occur until the blood is ingested by another Culicid.[1]

63. Notes on the Malaria-bearing Anophelines. — This section ought to have been placed in Chapter III, but has been relegated to the Addenda for the following reasons. Many works on malaria and on mosquitos give lists of the malaria-bearing Anophelines, but mostly without exact references to the literature in which the incriminating evidence was published. Hence early in 1910 I directed Mr W. R. Drawz, our Malaria Bibliographer (Tropical Diseases' Research Fund), to collect all the data which he could find; but the work could not be completed in time for the earlier chapters of this book; and he reports that even now, after several months' close search, some references have probably been overlooked. I am also much indebted to Mr R. Newstead and Dr J. W. W. Stephens, of the Liverpool School of Tropical Medicine, for their valuable assistance.

(1) So far as we can find, the following is a complete list of the Anophelines which have been *mentioned* in various books and papers as being capable of carrying malaria :—

(*Note.*—Only synonyms in small type.)

Anopheles albipes Theobald (1901), *vide*
Cellia albimana, (Wiedemann) (1821).

Anopheles albimana, Wied., *vide*
Cellia albimana, (Wiedemann) (1821).

[1] I am indebted to Professors Myres and Postgate, University of Liverpool, for advice regarding the Greek roots.

Anopheles argyrotarsis, R. D., *v de*
Cellia argyrotarsis, (Robin-Desv.) (1827).
Anopheles albitarsis, Arribalzagia (1891), *vide*
Cellia argyrotarsis, (Robin-Desv.) (1827).

Anopheles algeriensis, Theobald (1903).
Anopheles annulipes, Walker (1850), *vide*
Nyssorhynchus annulipes, (Walker) (1850).

Anopheles arabiensis, Patton (1905).
Anopheles barbirostris, Van der Wulp (1884), *vide*
Myzorhynchus barbirostris, (Van der Wulp).

Anopheles bifurcatus, (Linn.) (1758).
Anopheles bifurcatus, Meigen (1804), *vide*
Anopheles maculipennis, Meigen (1818).
Anopheles chaudoyei, Billet (*is not a spec.*), *vide*
Pyretophorus chaudoyei, Theobald (1903).
Anopheles claviger, Fabr. (1805), *vide*
Anopheles maculipennis, Meigen (1818).
Anopheles claviger, Meigen (1804), *vide*
Anopheles bifurcatus, (Linn.) (1758).

Anopheles cohaesus, Donné. (Doubtful species.)
Anopheles costalis, Loew (1866), *vide*
Pyretophorus costalis, (Loew) (1866).

Anopheles crucians, Wiedemann (1828).
Anopheles culicifacies, Giles (1901), *vide*
Myzomyia culicifacies, (Giles) (♀ non ♂) (1901).

Anopheles farauti, Laveran (1902). (Doubtful species.)

Anopheles formosaensis, Tsuzuki (1902)[1] (? Genus *Anopheles*).
Anopheles fuliginosus, Giles (1900), *vide*
Nyssorhynchus fuliginosus, (Giles) (1900).
Anopheles funesta, Giles (1900), *vide*
Myzomyia funesta, (Giles) (1900).
Anopheles funesta var. subumbrosa, Theobald (1903), *vide*
Myzomyia funesta, (Giles) (1900).
Anopheles funesta var. umbrosa, Theobald (1903), *vide*
Myzomyia funesta, (Giles) (1900).
Anopheles gambiae, Giles (1902), *vide*
Pyretophorus costalis, (Loew) (1866).
Anopheles grabhamii, Theobald (1901), *vide*
Cycloleppteron grabhamii, (Theobald) (1903).

[1] Said by Dönitz (*Zeit. f. Hygiene*, XLIII, 233, 1903) to be a variety of his *Myz. aconita*, which he names *cohaesa*.

Anopheles gracilis, Dönitz (1902), *vide*
Pyretophorus costalis, (Loew) (1866).

Anopheles jamesii, Liston (non Theobald) (1901), *vide*
Nyssorhynchus fuliginosus, (Giles) (1900).

Anopheles jesoensis, Tsuzuki (1902), *vide*
Myzorhynchus sinensis, (Wiedemann) (1828).

Anopheles kawari, James-Liston (1901), *vide*
Nyssorhynchus kawari, James-Theobald (1901).

Anopheles kochii, Dönitz (1901), *vide*
Cellia kochii, (Dönitz) (1901).

Anopheles kumasii, Chalmers (1900), *vide*
Myzomyia funesta, (Giles) (1900).

Anopheles leucopus, Dönitz (1901), *vide*
Nyssorhynchus fuliginosus, (Giles) (1900).

Anopheles listoni, Giles (1901) (? ♂ only), *vide*
Myzomyia culicifacies, (Giles) (♀ non ♂) (1901).

Anopheles listoni, Liston (1901), *vide*
Myzomyia listoni, Liston (non Giles) (1901).

Anopheles lutzii, Theobald (1901), *vide*
Myzomyia lutzii, Theobald (1905).

Anopheles maculipalpis, Giles (1902), *vide*
Nyssorhynchus maculipalpis (Giles) (1902).

Anopheles maculipennis, Meigen (1818).

Anopheles martini, Laveran (1902).

Anopheles metaboles, (Theobald) (1902), *vide*
Nyssorhynchus stephensi, (Liston) (1901).

Anopheles musivus, Skuse (1888), *vide*
Nyssorhynchus annulipes, (Walker) (1850).

Anopheles paludis, Theobald, *vide*
Myzorhynchus paludis, (Theobald) (1900).

Anopheles pharoensis, Theobald (1901), *vide*
Cellia pharoensis, (Theobald) (1901).

? *Anopheles pictus*, M'Donald, *vide*
Myzomyia hispaniola, Theobald (1903).

Anopheles pseudopunctipennis, Theobald (1901).

Anopheles punctipennis, Say (1823).

Anopheles pursati, Laveran (1902).

Anopheles quadrimaculatus, Say (1824), *vide*
Anopheles maculipennis, Meigen (1818).

Anopheles rossii, Giles (1899), *vide*
Myzomyia rossii, (Giles) (1899).

Anopheles sinensis, Wiedemann (1828), *vide*
Myzorhynchus sinensis, (Wiedemann) (1828).
Anopheles stephensi, Liston, *vide*
Nyssorhynchus stephensi, (Liston) (1901).
? *Anopheles superpictus*, Grassi, *vide*
Pyretophorus superpictus, (Grassi) (1900).

Anopheles tarsimaculata, Goeldi (1905) (not *Anopheles*).
Anopheles theobaldi, Giles, *vide*
Nyssorhynchus theobaldi, (Giles) (1901).

Anopheles treacheri, Leicester (1908).
Anopheles trifurcatus, Fabricius (1794), *vide*
Anopheles bifurcatus, (Linn.) (1758).
Anopheles turkhudi, Liston, *vide*
Myzomyia turkhudi, (Liston) (1901).
Anopheles vagus, Dönitz (1902), *vide*
Myzomyia rossii, (Giles) (1899).
Anopheles villosus, Rob.-Des. (1827), *vide*
Anopheles bifurcatus, (Linn.) (1758).

Anopheles vincenti, Laveran (1901).
Anopheles walkeri, Theobald (1901), *vide*
Anopheles bifurcatus, (Linn.) (1758).

Arribalzagia maculipes, Theobald (1903).

Cellia albimana, (Wiedemann) (1821).
Cellia albipes, Theobald (1903), *vide*
Cellia albimana, (Wiedemann) (1821).

Cellia argyrotarsis, (Rob.-Des.) (1827).

Cellia kochii, Dönitz (1901).

Cellia pharoensis (Theobald) (1901).
Culex bifurcatus, Meigen (1804), *vide*
Anopheles maculipennis, Meigen (1818)
Culex hyemalis, Fitch (1851), *vide*
Anopheles punctipennis, Say (1823).

Cyclolepppteron grabhamii, Theobald (1903).

Cyclolepppteron mediopunctatus, (Lutz M.S.), Theobald.
Myzomyia christophersi, Theobald (1902), *vide*
Myzomyia listoni, Liston (non Giles) (1901).

Myzomyia culicifacies, (Giles) (♀ non ♂) (1901).
Myzomyia fluviatilis, Christophers M.S. (1901), *vide*
Myzomyia listoni, Liston (non Giles) (1901).

Myzomyia funesta, (Giles) (1900).

Myzomyia hispaniola, Theobald (1903).
Myzomyia indica, Theobald (1901), *vide*
Myzomyia culicifacies, (Giles) (♀ non ♂) (1901).

Myzomyia listoni, Liston (non Giles) (1901).

Myzomyia ludlowi, Theobald (1903).

Myzomyia lutzii, Theobald (1905).

Myzomyia nili, Theobald (1904).

Myzomyia rossii, (Giles) (1899).

Myzomyia turkhudi, (Liston) (1901).

Myzorhynchus barbirostris, (Van der Wulp).

Myzorhynchus coustani, (Laveran) (1902).[1]

Myzorhynchus mauritianus, (Grandpré) (1900).

Myzorhynchus paludis, (Theobald) (1900).
Myzorhynchus paludis var. similis, Theobald (1901), *vide*
Myzorhynchus mauritianus, (Grandpré) (1900).
Myzorhynchus pictus, Ficalbi (1899), *vide*
Myzorhynchus pseudopictus, (Grassi) (1899).

Myzorhynchus pseudopictus, (Grassi) (1899).

Myzorhynchus sinensis, (Wiedemann) (1828).
Myzorhynchus tenebrosus, Dönitz (1902), *vide*
Myzorhynchus mauritianus, (Grandpré) (1900).

Myzorhynchus umbrosus, Theobald (1903).

Myzorhynchus ziemanni, Von Grünberg (1902).

Nyssorhynchus annulipes, (Walker) (1850).

Nyssorhynchus fuliginosus, (Giles) (1900).

Nyssorhynchus kawari, James-Theobald (1901).

[1] Theobald (V, 54, 1910) says that this is probably only *mauritianus* (Grandpré) with rubbed palpi.

Nyssorhynchus maculipalpis, (Giles) (1902).
Nyssorhynchus maculipalpis var. indiensis, Theobald (1903), *vide* *Nyssorhynchus maculipalpis*, (Giles) (1902).

Nyssorhynchus maculatus, (Theobald) (1900).

Nyssorhynchus stephensi, (Liston) (1901).

Nyssorhynchus theobaldi, (Giles) (1901).

Nyssorhynchus willmori, James (Theobald) (1904).

Pyretophorus ardensis, Theobald (1905).

Pyretophorus chaudoyei, Theobald (1903).

Pyretophorus costalis, (Loew) (1866).
Pyretophorus costalis var. melas, Theobald (1903), *vide* *Pyretophorus costalis*, (Loew) (1866).

Pyretophorus jeyporensis, James (1902).
Pyretophorus jeypurensis, Theobald (1903).

Pyretophorus myzomyifacies, Theobald (1907).

Pyretophorus sergentii, Theobald (1907).

Pyretophorus superpictus, (Grassi) (1900).
Stethomyia fragilis, (Theobald) (1903), *vide* *Anopheles treacheri*, Leicester (1903).

Stethomyia nimba, Theobald.

(2) I now add some notes on the species regarding which we have succeeded in finding some incriminating evidence. That evidence is of three degrees of value:—(*a*) experimental proof that certain individuals of a species are capable of developing all the stages of any or all the species of the human *Plasmodia*, or of causing infection in man, or of both; (*b*) proof that a species may contain some of the stages of the parasites (zygotes); and (*c*) apparent correlation between the numbers of a species and the local prevalence of malaria. Here (*b*) is not conclusive because, though the zygotes may develop to a certain degree in an insect, it does not follow that they come to maturity; and (*c*) is not worth much because, as we showed in the case of *Myzorhynchus mauritianus*, a species may abound in a malarious locality, although the disease is really being spread by quite another one. Lastly, even (*a*) proves only that the *variety* concerned

in the experiments is culpable, because, for instance, we failed in infecting English *A. maculipennis*, which certainly carry in most other places. The reader should also note that the breeding-waters selected by larvae in some places need not necessarily be the same as those selected by the same larvae elsewhere.

Anopheles algeriensis.—Ed. and Et. Sergent state that they found protospores in the salivary glands of the two individuals examined by them [1905]. They also state that a violent epidemic of malaria occurred in the villages of Thiers in Algeria, where it was not possible to find any other Anopheline except this one. Occurs in Algeria, and the Sergents say that it haunts *les collines saheliennes*, and the plains of the littoral.

Anopheles arabiensis.—Apparently incriminated by W. S. Patton [1905]. Literature not available. Protospores found in it. Distribution: Arabia and Aden. See Stephens and Christophers [1907, p. 156].

Anopheles bifurcatus.—There are two species of Anophelines under the name of claviger, *A. maculipennis* Fabr. and *A. bifurcatus* Linn. Both appear to have been incriminated by the Italians by cultivation of the parasites in all their stages, and also by inoculation of healthy persons (see section 17, cases 2, 3, 4); but it is not always clear which species is referred to. We gather that *A. maculipennis* is the one concerned in the experimental inoculations, and generally in the cultivations. But Bignami and Bastianelli also incriminated *bifurcatus* by cultivation. Grassi says [1901] that he found it much more difficult to work with *bifurcatus* than with *maculipennis*, owing to the small size of the former, but found zygotes in thirteen out of sixteen insects, and adds (page 121, German edition) that parasites were found in *bifurcatus* and *maculipennis* in the Maccarese district. Janscó also [1904, 1905 and 1908] gives experiments with *A. claviger* without stating which *claviger* he referred to; but we gather that he was referring to *maculipennis*. Probably *bifurcatus* can be safely claimed as a carrier.

It is generally distributed over Europe and North America. In England it is certainly the commonest of the three Anophelines, the other species being *maculipennis* and *nigripes*. Grassi states that it occurs in forests and breeds in small collections of fresh water such as wells and in rot holes in trees; that it occurs seldom in houses and stables, and that when one is bitten in forests it is generally by this species. It bites more quickly than *A. claviger*, and in the daytime,

and enters habitations at night. Newstead says that it is the commonest Anopheline in the New Forest in England, where the larvae occur in large numbers in the shallow pools and in the cattle footprints. In the marshes of Cheshire it is also abundant, occurring chiefly in shallow, terrestrial waters, sometimes in company with the larvae of *Culex*.

Anopheles formosaensis.—The generic position of this species has not yet been definitely fixed. Incriminated by J. Tsuzuki [1902], who followed completely the development of the malignant parasites in it, but did not inoculate man with it. Occurs in the whole island of Formosa. Tsuzuki apparently distinguishes two varieties, one of which is common in the north and the other in the south of Formosa.

Anopheles maculipennis.—Incriminated by the Italians, see under *A. bifurcatus*; also by A. Van de Scheer and Van Berlekom [1900], by Schaudinn [1903], by Hirschberg [1904], Husson [1907], and by Janscó [1904, 1905, 1908], and others. The proof is by complete cultivation of the parasites, and also by inoculation of man by the Italians and by Janscó. Common and generally distributed throughout Europe, including Russia and Scandinavia. It appears also to be generally distributed in the United States of America, and to be common in many parts of Canada. It is chiefly a domestic insect, found in houses, sheds, stables, and hen coops, etc. This habitat also obtains to a very large extent in Great Britain. The larvae occur chiefly in open terrestrial waters in marsh lands, in forests and boggy places. Theobald states that in Britain the food of both sexes is entirely vegetable. Ficalbi, on the other hand, says it is very troublesome to man in Italy.

A. pseudopunctipennis.—Incriminated by S. T. Darling [1910], who found zygotes in four out of thirty-one insects fed on cases of the malignant parasites, and also protospores in the glands of one insect. He thinks that it is only slightly concerned in the transmission of malarial fever in the Canal Zone. Occurs in Panama, Grenada and New Mexico. Darling says that it is one of the two commonest species in the Canal Zone, and breeds in most terrestrial waters.

A. tarsimaculata.—Incriminated by S. T. Darling [1910], who found zygotes in three out of five insects. Occurs in Panama and South America. It is doubtful if this species is referable to the genus *Anopheles*.[1]

[1] Theobald (V, 68, 1910) places it as a synonym of *Cellia albimana*, (Weid.).

Cellia albimana.—Incriminated by S. T. Darling [1910], who found zygotes in thirty-six out of fifty insects and protoclasts and protospores in three. Occurs in Panama, Brazil, British Guiana and in the West Indies. Theobald (vol. iii. p. 111) states that Dr St George Gray says that this Anopheline will bite at any time of the day or night; that the breeding-grounds are extremely varied, such as collections of water, especially full of reeds; and records also that he has found larvae in brackish water in a lagoon shut off from the sea. He states, however, that he has never found the larvae in water barrels or similar receptacles in towns. Darling [1909, 1910] says that this and *pseudo-punctipennis* are the commonest mosquitos in the Panama Canal Zone, and breed in almost any terrestrial water. It carries both malignant and tertian malaria. Theobald, in his "Mosquitos of Jamaica," also mentions rivers and large swamps and irrigation water, etc., as breeding-places.

Cellia argyrotarsis.—S. T. Darling [1910] says that a zygote was found in one individual at Panama. Occurs in South America, the West Indies, Panama, but appears not to be an important carrier. From the records we gather that this is a less common species than *C. albimana*, to which it is very similar (possibly only a variety).

Cellia pharoensis.—Newstead, Dutton and Todd [1907-1908] say that "malaria parasites were seen to develop in this mosquito at Boma." The insect was common at Ismailia, and has been reduced simultaneously with malaria by operations since 1902 (section 53). Pressat [1905] considers it to be malaria-bearing, but gives no evidence. Occurs in East, West, Central and Northern Africa. It is recorded also from Palestine. Theobald (vol. i., 1901) says that this mosquito occurs during the month of April in Mashonaland and during January in Egypt. I found the larvae in small swamps of almost fresh water caused by seepage from the fresh-water canal at Ismailia, also in an ornamental fountain and in water-cress beds. Not seen in sewage cisterns. Willcocks say that it is a domestic mosquito, both in the larval and adult stages. The adults enter houses in order to obtain blood. In the open the females bite most viciously at sunset. The water in which the larvae live may be brackish. Larvae, in various stages of development, placed in water containing 1·78% common salt, die in less than twenty-four hours, but in water containing 1% common salt they live from two to three days, but become sluggish in their movements and appear to feed very little or not at all.

Myzomyia culicifacies.—Incriminated by Stephens and Christophers [25th April 1902]. Experimentally it carries quartan as far as the protospores, and the other two species as far as the zygotes. In nature frequently found to be infected in 4·6% at Mian Mir, and in 8·6% at Ennur, near Madras. Widely spread in India, where it is one of the principal carriers. Breeds in pools in sandy river-beds, sluggish irrigation channels, ditches, muddy tricklets, edges of rivers, etc.

Myzomyia funesta.—Proved by Ross, Annett and Austen [1900] to carry quartan (one out of five insects) and mild tertian (one insect) in West Africa. Further confirmed by Daniels [1900, p. 41] for malignant. Stephens and Christophers [1901] found what appeared to be this Anopheline in the Duars, in India, but Theobald subsequently did not admit the identity. Also observed by Dutton and Todd, who report that malaria was seen to develop in it at Lusambo [1907]. Occurs in Central, Western and Southern Africa, Sudan and the Philippine Islands. Breeds in clear water in small springs and edges of streams and rivers. Daniels says it cannot thrive in stagnant waters, and that it may be found in marshes, but only where the water is kept fresh by springs during heavy rainfall. Stephens and Christophers say that it breeds in running waters, such as sluggish irrigation channels, ditches, etc. It frequents houses and does not leave them in the daytime. It feeds preferably at night, especially in the early hours of the evening, but also at other times, even in daylight, both when free and in captivity.

Myzomyia hispaniola.—Incriminated by Sergent brothers [1905], who dissected fourteen in 1904 without result, but in 1905 found protospores in one of three dissected. Occurs in Spain, Teneriffe, Algeria. The Sergents say that it abounds above all in valleys of hilly, broken regions, but that it is found at a distance of some kilometres in the plains in the *débouche* of these valleys, and add that this Anopheline is like *algeriensis* but is little domestic. It frequents houses only during the night, and does not remain after having bitten its victim.

Myzomyia listoni.—Incriminated by Stephens and Christophers. In this species protospores were readily found in the Bengal Duars, but only in nature. Also by Kinoshita [1906], who says it can be infected from 50 to 98% in Formosa. Occurs in India, Japan, Federated Malay States. Inhabits running waters, swiftly-flowing

streams, sluggish irrigation channels, ditches, muddy tricklets and edges of rivers.

Myzomyia lutzii.—Lutz [1903] says that he is convinced that this mosquito carries malaria, but gives no further evidence. Oswaldo Cruz (section 47) suspects it. Occurs in Brazil, British Guiana. Lutz who paid considerable attention to the bionomics of this mosquito, states that it is found widely distributed in various places along the chain of mountains in the region of San Paolo and Santos. It breeds in Bromilaceous plants.

Myzomyia rossii.—I failed in infecting this species in Calcutta in 1898-1899. Stephens and Christophers [1902] obtained experimentally quartan and malignant zygotes in a few individuals. Never found them infected in nature. They add that zygotes were found in nature by Captain James, but sporozoits never in nearly 1,000 dissected. W. T. de Vogel [1909] found zygotes in two insects bred from larvae grown in salt water (1·3%), but not in those bred in fresh water. C. A. Bentley also finds them in sea water; and failed to discover the parasites in 425 dissected. Occurs in India, Ceylon, Malay States, China, East Indies, Philippine Islands, and Java. Breeds in foul puddles near habitations, clean clear water puddles without much algae, and often turbid with suspended matter, boats and puddles near houses, shallow muddy rivers, rice fields and cultivation of all kinds. Theobald states that Mr E. E. Green (*Tropical Agriculture*, vol. xxvii. p. 84, 1906) considers that it is a malaria carrier in parts of Ceylon. He found the larvae breeding in a brackish lake at Batticaloa, and also in the cocoanut estates, in the small water holes used for watering young plants, and in chatties sunk at the roots of plants. I have often thought that the comparative healthiness of India may be due to the fact that this species ousts more dangerous ones. Vogel's suggestion is interesting.[1]

Myzomyia turkhudi.—Incriminated by Stephens and Christophers [1902]. Found to carry, experimentally, malignant tertian zygotes. Occurs in India; and found in large pools in quarries, stony and shallow running waters.

Myzorhynchus barbirostris.—Proved by Stephens and Christophers [1902] to develop malignant tertian experimentally. Their first experiments had been negative. Occurs in India, Malay Peninsula. Stated

[1] See also section 65 (8).

to have been found at Old Calabar (?). Breeds in running waters with much weed and algae, swamps, and deep water with much aquatic vegetation.

Myzorhynchus sinensis.—Tsuzuki [1902] proved that the parasites develop in all stages in this mosquito and that it causes infection in a healthy man. Also that quartan parasites develop in many of these mosquitos; but he did not infect man with this species of parasite. Kinoschita cultivated mild tertian parasites in seven out of sixteen mosquitos, but quartan only at low temperatures, and malignant parasites not at all. Occurs in Formosa, Japan, China, Federated Malay States. Habits not described.

Myzorhynchus umbrosus.—Incriminated by Malcolm Watson. Experiments not mentioned, but certainly a carrier. Occurs in the Federated Malay States. Breeds principally in the flat country, but not in flowing water. Abounds in thick jungle which it will not willingly leave, so that removal of jungle round plantations reduces malaria (section 57).

Nyssorhynchus annulipes.—Kinoshita [1906] developed malignant, only at high temperature without oscillations, in three out of five mosquitos. Occurs in South West Queensland, Australia, Formosa. Dönitz says it occurs in the Bismarck Islands. Breeds both in fresh and in salt water (1,040 sp. gr.), and will live for a month on dates.

Myzorhynchus fuliginosus.—Incriminated by Stephens and Christophers [1902] and Adie [1903]. Stephens and Christophers say that it occurred in Mian Mir in small numbers, and think that it is not an active agent. They were able to infect it experimentally for malignant tertian (zygotes), in one case sixty-six medium and ten small zygotes. It developed also quartan up to zygotes, experimentally. Occurs in India, Federated Malay States. Breeds in puddles and pools with much algae, common in stream beds, water trickling over rocks and lakes with weedy margins. Giles and Liston say that as a rule this species prefers open water and sunlight.

Nyssorhynchus maculipalpis. — Stephens and Christophers found zygotes experimentally in this mosquito, which they called *N. jamesii.* Occurs in India, Mauritius, Mashonaland. Scarce in Mauritius. Breeding in marshes on sea coast, and at 1,400 feet above sea.

Nyssorhynchus stephensi. — Stephens and Christophers [1902]

developed zygotes of malignant tertian in it, experimentally. W. G. Liston [1908] at Bombay found 25% infected with malaria. C. A. Bentley, in the same city [1910], found zygotes in 30 and protospores in 8 out of 404 dissections. Occurs in many parts of India. Breeds not only in open, clean terrestrial waters, but in deep wells, iron cisterns, filter beds, garden tanks, and even small vessels (section 59).

Nyssorhynchus theobaldia. — Stephens and Christophers [1902] developed malignant tertian and quartan to zygotes, experimentally. Occurs in India and Aden Hinterland. Breeds in running, stony and shallow water with much weed and algae.

Nyssorhynchus willmori. — Incriminated by Malcolm Watson (section 57). Details of experiments not given, but the insect appears to be certainly a carrier in the hilly land in the Federated Malay States. Also Daniels [1909]. Occurs in Federated Malay States, Kashmir, Ceylon. Breeds in rapidly running streams, and cannot be reduced by open drainage. The larvae were found in the clear puddles formed by a spring at a height of 4,800 feet in Kashmir (Theobald, vol. iii. p. 102).

Pyretophorus chaudoyei.—Billet states that this mosquito occurs exclusively in places where there is much malaria in the Saharian Oases in Algeria. He had no opportunity of finding zygotes in the insects, but is sure that it is a carrier. Theobald (vol. iii. p. 70) says that protospores have been found in it. Occurs in Algeria, Touggourt in Algeria, and southern posts in Sahara. Breeds in water containing a higher percentage of salt than normal sea water. Larvae are found in little isolated pools or ponds, which under the influence of active solar radiation greatly increase in the percentage of salt, giving at the end of summer a percentage of 40 grammes per litre. (Foley and Yvermault, *Bull. Soc. Path. Exot.*, 1908, i. iii. pp. 172-173.)

Pyretophorus costalis. — Incriminated by Ross, Annett, Austen [1900] to carry all three species in 27 out of 109 insects. Also by Stephens and Christophers [1900] and by R. Ross in Mauritius [1908]. Common and widely distributed over the African continent, Madagascar, Réunion and Mauritius, but not in many neighbouring islands which are non-malarious. Breeds in stagnant terrestrial waters often without much weed. Appears to prefer the sea coast in Mauritius, but was found in marshes up to 1,700 feet, where, though scanty, it yet caused malaria. Breeds in pools in dried-up beds of hill streams,

and in weedy margins, etc. In Mauritius prefers verandas to inner rooms, and bites in the open, especially after 11 P.M. Takes shelter from wind, but wanders far on windless nights.

Pyretophorus myzomyiafacies (?).—Incriminated by Ed. Sergent (section 52), who says that the protospores have been found in it. Occurs in Algeria, especially in broken, hilly valleys.

Pyretophorus superpictus.—Grassi seems to think that it is a malaria carrier [German translation, p. 219, 1901], but apparently gives no evidence. Also Bignami and Bastianelli think that it is a carrier. Occurs in the South of Italy, Spain, Greece, Algeria, and (?) Africa and India. Carries *Filaria immitis.*

This concludes the Anophelines regarding which we have been able to find any reliable experiments, but other species are mentioned as being possible or certainly carriers, but without giving the evidence. Thus Oswaldo Cruz (section 47) says that *Cycloleppteron mediopunctatum* Theobald and Lutz, *Cycloleppteron intermedium* Chagas, and *Arribalzagia pseudomaculipes* Chagas, "are undoubtedly carriers, as shown experimentally." C. Daniels, in a letter to me of the 17th June, says that *C. kochii* is the constant in badly malarial suburbs in the Malay States, and is absent or scanty in other places. *A. treacheri* (*Stethomyia fragilis* Theobald) was the only mosquito which he could find in badly malarial jungles. He adds that it was difficult to make any experiments with these two species, as they will not live in confinement, and says: "The trouble with these, as with several other mosquitos, is that whilst you cannot absolutely include them because there is no positive proof (experimentally), you cannot exclude them because there is no negative proof (experimentally), and the circumstantial possible evidence is strong." Laveran mentions two species which he calls *pursati* and *martini* as being carriers; but the entomologists appear not to accept them as species. Obviously much more exact work requires to be done even on this point, which is so important for public prevention.

C. S. Banks [1907] states in a lengthy paper that he cultivated plasmodia in *Myzomyia ludlowii* Theobald, a Philippine Anopheline which breeds in rivers and streams and also in sea water, and that he inoculated a healthy person by its means; but certain points in his description suggest doubts as to the nature of the bodies which he considered to be malaria parasites.

64. Examples of Legislation. — Notes on legislation have already been made in section 40 (4) and 59 (1). The object of such legislation is to give the Health Department power (*a*) to enter private premises (including houses, yards, gardens, lands and estates) in order to search for mosquitos, flies, rats and other annoying or dangerous vermin; (*b*) to destroy such vermin when found; and (*c*) to prevent the breeding of them. Another object is to compel owners or occupiers to take reasonable precautions against such breeding. But at the same time the rights and properties of the owners or occupiers have to be duly guarded. Most civilised malarious countries now possess Health Acts, more or less based upon precedent, and these contain clauses against "nuisances." But the word "nuisance" is not always defined so as to include the breeding of vermin. In such cases, then, it suffices merely to extend by act the definition of the word — if possible, to include all vermin as well as mosquitos. It was my duty in Mauritius to consider the local Ordinances carefully for this purpose; and I drafted the following clauses, which were scrutinised and accepted by the Health Department and by the Procureur-General, and then submitted to Government. (The first clause here given was meant to be inserted in the definition of "nuisance" contained in the Health Ordinance already in existence):—

"All collections of water, rubbish, refuse, ordure, or other fluid or solid substances, and all other conditions which permit, or facilitate, or are likely to permit or facilitate, the breeding or multiplication of animal or vegetable parasites of men or domestic animals, or of insects or other agents which are known to carry such parasites, or which may otherwise cause or facilitate the infection of men or domestic animals by such parasites.

"It shall be lawful for any sanitary authority, or any person deputed by him in writing, to take immediate steps to destroy mosquito larvae on any premises where they may be found, and to take such action as may be necessary to render any pools or accumulations of water unfit to be breeding-places for mosquitos.

"When such pools or accumulations of water lie on premises under the charge of a public body or corporation, they shall not be dealt with as above provided, unless due warning has been given in writing to such public body or corporation, and no action has, within reasonable delay—not to be less than twenty-four hours—been taken by them. In such cases the expenditure incurred shall be borne by such public body or corporation.

"Any owner or occupier who shall object to pools and collections of water on his premises being dealt with as above provided, shall within twenty-four hours submit his reasons to the sanitary authority,

who, after enquiry, shall order such action to be taken as he shall consider necessary to meet the provisions of this Ordinance. Should the objections be rejected, the measures originally ordered shall be carried out at the expense of the said owner or occupier.

"It shall not be lawful for any owner or occupier to allow mosquitos to breed on his premises, or to allow the presence on such premises of any receptacle in which water is kept or may collect, unless such receptacles are properly protected from access of mosquitos, or unless the water they may contain is treated in such a way as to prevent the breeding therein of mosquitos, nor shall such owner or occupier allow on his premises any conditions which may, in any way, be favourable to the breeding of mosquitos.

"Trees on all premises shall be at all times kept freely lopped to the satisfaction of the sanitary authority by the owner or occupier, and no trees shall be allowed to grow within 10 feet from any dwelling-house. The sanitary authority may, in writing, direct the said owner or occupier to carry out the above provision within a reasonable delay, not to be less than forty-eight hours, and, in case of non-compliance, the trees shall be lopped or cut down at the expense of the owner or occupier.[1]

"It shall be lawful for the Director of the Health Department to make such regulations as may be necessary to carry out the provisions of this Ordinance.

"It shall be lawful for the Director of the Health Department, in any case when the owner or occupier of any premises is liable for the expense of any measures carried out on his premises, to relieve such owner or occupier from the said expense, if, after enquiry, the Director is satisfied that such owner or occupier is not in a position to incur such expense. In such cases the expenditure shall be borne by Government."

Clauses of penalties follow.

Sir Rubert Boyce gives [1910] the laws passed in several West Indian Colonies. From my experience (in 1896 I was a member of a committee which reformed the municipal laws in the station of Bangalore in India), I do not think that all of them are very good or practicable; and Boyce admits that they have often become a dead letter. But I quote some of the clauses as examples.

"The occupier or owner of any premises shall keep such premises free of stagnant water liable to breed mosquitos, and the presence of mosquito larvae in any collection of water, wherever situated, shall be sufficient evidence that such water is stagnant." Accepted in most of the colonies—a simple and inclusive clause.

[1] Some specific provision ought to be made to enable the sanitary authority to fill up with concrete, or otherwise to treat, holes and hollows in trees which breed, or are likely to breed, mosquitos; and also to compel owners to cut insanitary undergrowth.

"The owner, or when required by the local authority, the occupier of every lot of land situate in a town or village district shall effectually drain the lot, and for that purpose shall (1) make such dams and drains on the lot as may be necessary for effectually draining the lot; (2) fill up all irregularities on the surface of the lot, and adjust the surface thereof, and if necessary raise the level of the surface thereof, in such a manner (*a*) that the water received on the lot may flow into the drains without obstruction, (*b*) that no water can remain on any portion of the surface of the lot other than the drains, and (*c*) that the surface of the lot does not remain swampy. Provided that where the swampy state of any lot in any such district is occasioned by the main drains into which the drains of the lot discharge not having a sufficient outfall or a sufficient capacity to carry off all the water discharged into them, the owner or occupier of the lot shall not be liable under this section to raise the level of the surface of the lot if the level of such surface is as high as the average height of the level of the land surrounding such a lot for a distance of 20 roods (? 20 rods = 110 yards. A rood is a square measure); and *provided* that any owner may, with the consent of the local authority of the district in which the lot is situated, have a pond on the lot." *George-town, British Guiana.*

"All vats, tanks, or other vessels shall be screened with mosquito-proof wire-netting or other suitable material, so as to prevent the entrance into or exit of mosquitos from such vats or tanks or other vessels." *British Guiana.*

Such regulations should, if possible, be included in the general Health Act, and not be left to the bye-laws of local bodies, which are often very incompetent. In my opinion British administration is generally much wanting in discipline, and tends to neglect the health and lives of the people for the sake of antiquated notions about the liberty of the subject.

65. Notes.—I will conclude this book with some miscellaneous notes on points of interest which have been studied while it was in the press.

(1). *The thick-film process.*—For ordinary microscopic preparations, 1 c.mm. of blood is spread out thinly over, say, 4 sq. cms. of area, and then examined either fresh or stained — at the cost of much time (section 18 (9)). Consequently I proposed the following method [1903]. A quantity of blood, say 1 c.mm., is spread over only about 1/4 sq. cm. of area, and allowed to dry. The haemoglobin is then washed out with water, and the residue, consisting of parasites, leukids, micrids and the stromata of the haematids, is stained by any appropriate method. We can thus search 1 c.mm. of blood in about 1/16

the time, or less, required for an ordinary preparation — but the parasites, especially *P. vivax*, are apt to be overlooked unless the observer trains himself in the work. Originally I washed out the haemoglobin with water and then fixed and stained the residue as usual; but later I obtained better results by washing out with eosin solution, washing this off, and then passing over a weak solution of methylene blue. R. Ruge, however, advocates the former procedure [1903], and L. Rogers [1908, p. 17] prefers first to fix and stain, and then to wash out the haemoglobin with a very dilute solution of acetic acid—which leaves the outline of the haematids without obscuring the view. All these methods are useful. Sediment and overstaining of the stromata are to be avoided, and the film should not be too thick.

(2). *Enumerative methods.*—Very rough ones are now in use. We count the number of parasites in a given number of fields of an ordinary preparation—giving an enormous possible error, because we do not know the amount of blood in each field. Or we count the number of parasites found near a given number of leukids, the latter being counted by a haemocytometer. Here the total estimate is the product of the two partial counts; and if the errors contained in the latter are both positive or both negative (which should occur in half the estimations), the total error may again be enormous. Direct counting by the haemacytometer is far from easy in the case of small parasites, owing to the depth of the fluid examined.

Since the beginning of this year (1910), the Advisory Committee of the Tropical Diseases' Research Fund (Colonial Office) have granted considerable funds for the accurate study of cases of malaria in Liverpool, and Dr David Thomson and myself have commenced this study by elaborating improved "enumerative methods" based on my thick-film process. A measured quantity of undiluted blood is made into a thick-film preparation, and the total number of parasites contained in the *whole* of it is carefully counted. The quantity of blood used is measured by means of a graduated pipette or capillary tube. It is necessary that this tube should have a very fine calibre to allow a small quantity of blood, such as 1 c.mm., to occupy a sufficient length of the tube to permit of accurate measurements being made. Thus a tube of 180μ in diameter, and of 4 cm. in length, will contain 1·018 c.mm. (say 1 c.mm.) of blood. Dr Wakelin Barratt points out that any one can make a suitable pipette by drawing out a fine capillary glass tube, measuring its calibre by the microscope, and calculating the length required to contain the given volume (area of

circle = square of diameter x by 0·7854, and area of ellipse = product of the two diameters x by the same figure). We are, however, making special tubes, measuring 1/4 c.mm. or more.

Mr M. Greenwood, Jr., of the Lister Institute, informs me that the percentage of error is $e\% = 67{\cdot}449 / \sqrt{n}$ where n is the number of bodies actually counted in the measured quantity of blood. Thus the error = 1% if we have counted 4,550 bodies, 2% for 1,138 bodies, 5% for 182 bodies, and 10% for 46 bodies. We must therefore always count enough bodies (parasites or leukids) to reduce the error to any required percentage. Hence the measured quantity of blood should be small when the parasites are numerous, and large when they are scarce. We should perhaps allow 5% error for the measurement of the blood, especially when very small quantities are taken. First sample should be rejected, and fixing before staining is demanded in order to avoid loss. The method is excellent for leukids.

(3). *Some results.*—Applied to a case of sleeping sickness this method disclosed, besides other facts, a regular periodic rise and fall in the number of trypanosomes occurring every seven to eight days — a phenomenon apparently not previously detected (*Proceedings of the Royal Society*, June 1910). With twenty-five cases of malaria some of our results are, briefly, as follows :—

When fewer than about 1,000 per c.mm., the sporids are not always numerous enough to cause marked rise of temperature (section 18 (8)). They may remain present in very small but detectable numbers during every day between relapses. A rise in their numbers to about 1,000 or more is generally accompanied by a febrile relapse and followed by a fall in haemoglobin. A fall in the number of malignant sporids may be followed by such an abrupt and high rise that the said fall cannot be due to the death of the parasites, but must be caused by their retirement from the peripheral blood — thus proving the hypothesis of Marchiafava and Bignami. Relapses may occur without a single crescent being found even after daily thick-film examinations made during twenty-nine days; and the number of sporids counted during such relapses cannot be explained by parthenogenic reproduction, unless we suppose that each crescent can produce some hundreds of spores. A rise in the sporid curve is often followed after eight to ten days by a corresponding rise in the crescent curve, suggesting that the latter require this period for development from the former; but sometimes no crescents at all are produced, even without quinine; or else the gamogenesis commences or ceases suddenly. The rise of the

crescent curve may be very rapid and continuous. The fall may be equally rapid at first, but often alternates with short rises—suggesting continuous death and reproduction; but later the fall tends to be more gradual. Quinine (even in 2 gramme daily doses in a boy), soamine and methylene blue had no very decisive effect on the crescents when once formed. The crescents have never numbered more than 1/8 the highest number of sporids. Of the latter (malignant), 300,000 was the largest number found per c.mm. G. C. E. Simpson, working with us, has demonstrated parallel fluctuations of urobilin in urine and faeces. Faradic and galvanic currents applied to the spleen did not increase the number of crescents in the peripheral blood, and X-rays to the spleen did not prevent a relapse. (Compare Chapter IV.)

The method gives at the same time much more accurate diagnosis, and is useful for obtaining the parasite rate (section 51).

(4). *S. T. Darling* has recently done some good enumerative work at Panama, partly published [1909] and partly just communicated to me by letter [1910]. He counted the number of crescents in patients (by comparison with leucocytes), fed Anophelines on them, and then counted the zygotes in the insects; and estimates that the mortality of the parasites in the stomach cavity is 97% (section 18 (1)). *A. albimana*, bred in laboratory, weighed 0·0008 grammes before feeding and 0·0016 grammes after a moderate blood meal. He takes the average blood-meal to weigh about one milligramme, and from the number of gametids counted in the blood, he estimates the number which must have been ingested during the meal. This he compares with the number of zygotes actually found. For example, he estimated that one mosquito should have contained 1,632 zygotes after three feedings: but it contained only fifty. He thinks that fully half the ingested gametids are captured by leukids in the insect's stomach—as I showed in 1895. In one mosquito he found 168 zygotes. He concludes that if the gametids in a patient's blood are less than about 12 per c.mm. they will not be numerous enough to infect mosquitos. After experiments on "a number of patients," he thinks that 30 grains of quinine taken daily will reduce the number of crescents (section 23). We have seen, however, that the crescent curve may fall quite irregularly, and sometimes vary rapidly, without any quinine.

At my suggestion my brother, *E. H. Ross*, recently carried out the following experiments at Port Said, in parts from which mosquitos have been entirely banished: 344 *Culex fatigans*, males and females,

were liberated from bottles in houses free from mosquitos, and search was made to ascertain what became of them. Only 54, or 16%, were found, mostly dead, during the next day or two, after which the remainder disappeared entirely. Only three persons were bitten after the experiment—suggesting that the biting ratio (section 29 (3)) is very low, say 1/50.

(5). We have found three more successful mosquito inoculations of man, bringing the number up to 38 (section 17).

Case 36. J. Tsuzuki [1902].—Source, benign tertian. One of many infected *Myzorh. sinensis* bit healthy subject in Sapporo, Jeso, Japan, which is free of malaria on 24th August (?) 1901. Fever and parasites on 31st August.

Case 37. N. Janscó [1903].—Source, malignant. *A. claviger* (? Meig.) bit subject on 6th December 1907 in Hungary. One gramme sulphate of quinine given on seventh and eighth day after inoculation. Fever and parasites on 18th December.

Case 38. *Ibid.*—Same source and one *A. claviger.* Subject given same dose of quinine on eighth, ninth and tenth days after inoculation on 20th and 25th October (? year). Fever on 4th November and parasites on the 5th.

Janscó also records three interesting negative cases obtained at the same time. *Case* 1 received 1 gramme of quinine daily from the second to the fourteenth day after inoculation, and remained well during nine months' incubation. *Case* 2 received 1/2 gramme every morning on an empty stomach from ten hours before inoculation to the thirteenth day afterwards, and remained well during one month's observation. *Case* 3 received 1·5 grammes quinine on fourth, fifth, ninth, tenth, fourteenth and fifteenth days after inoculation, and had no fever during one month's observation.

M. Glogner [1905] records two cases which he thinks were due to inoculation during vaccination.

(6). With reference to the attitude of Anophelines, Mr Theobald informs me that the only species known to him of which the larva suspends itself in water like a Culicine larva is *Myzomyia azriki* Patton, 1905; and that *Myzomyia culicifacies* is the only one of which the adult has an abnormal attitude. So far as he knows the larvae of all the species have no siphon.

(7). Regarding the enumeration of mosquitos (section 29 (4)), I forgot to mention a method used at Panama and elsewhere which consists in keeping note of the number of breeding-places found from time to time. It is, of course, a very vague method, but has the advantage of being easily practised by the working gangs or moustiquiers. Such records give them no additional trouble and serve for a check upon their work.

(8). A. Eysell in the *Archiv. für Schiffs und Tropen Hygiene*, Band XIV., 1910, argues that *M. rossii* is the same as the malaria bearing mosquitos of Kinoshita and also of Schüffner.

(9). Colonel W. G. King, the distinguished Sanitary Commissioner of Madras and Burma, informs me that from estimates which he has made regarding the cost of death and of sickness in India, he finds that the death of an adult costs Rs.20; of a child Rs.7; while a month's sickness of an adult costs Rs.9. and of a child Rs.4·5. As there are quite 1,000,000 deaths from malaria every year in India, with a corresponding amount of sickness, the total cost of the disease may be roughly computed from these figures.

ADDENDA TO THE SECOND EDITION, 1911

(10). *Miscellaneous Notes.*—Dr Malcolm Watson's book on the Prevention of Malaria in the Federated Malay States, referred to in section 57, has recently been published by the Liverpool School of Tropical Medicine, price 7*s.* 6*d.* It contains many details which could not be given in section 57, is fully illustrated, and should be read by all practical health officers. It has been very favourably reviewed by W. G. King in *Nature* of the 20th April 1911, and by others elsewhere. In *Nature* of the 9th February, however, a correspondence between C. A. Bentley and M. Watson was published, in which the former argued that the improvement at Klang and Port Swettenham was due not to the drainage, but to removal of overcrowding, resulting from the cessation of the construction works; but the latter easily disposed of this fancy. I mention the matter here only because Dr Bentley's letter has been published elsewhere without Dr Watson's reply; but may add that the former contains a fault very common in such literature. It begins by suggesting a hypothesis as a possible explanation; then accepts it without the smallest proof; and lastly concludes that the other explanation of the facts (which happens to be the true one) is

"an absurd fiction." Dr Watson has studied malaria prevention by all methods impartially during ten years over a large area of difficult country, and his conclusions deserve the closest attention.

The results of the *enumerative methods* mentioned above have been published in the *Proceedings of the Royal Society*, and in full in the last four numbers of the *Annals of Tropical Medicine and Parasitology* (Liverpool), in a series of articles by Drs David Thomson, Fantham, Simpson, John Thomson, and myself. The work is of course mostly pathological, and a review of it will be found in my paper in the *Transactions of the Society of Tropical Medicine and Hygiene*, March 1911. The most important findings in connection with malaria are evidence of the theory of the parasitic invasion given in Chapter IV, and of the production of crescents mentioned in (3) above. These bodies are not directly affected even by large doses of quinine, and we have not found any other drug which destroys them quickly. Even for the asexual forms, about 2 grammes of quinine can be given daily for four or five days without causing total disappearance in 1 c.mm. of thick film—so that "prevention by treatment" probably requires much larger and more continuous dosing than is imagined at present. This should not discourage the measure, but should demand greater thoroughness in its application. The failures referred to by Watson and many others are doubtless due to insufficient dosing. In my opinion, infected persons had much better be kept at complete rest (even in bed) during one month's vigorous cinchonisation. Ehrlich's *606* has been well reported on for malaria, especially in Algeria, but the evidence is far from complete. We are now seeking other improvements.

Mr Edie, chemist to the Liverpool School of Tropical Medicine, has made at my suggestion *larvicide pills* of cyanide of potassium contained in a solid floating medium. Experiments on larvae show that this poison is extremely deadly to them in solutions of such tenuity, that a man would have to drink 4 gallons (nearly 20 litres) of a solution at a time before receiving a lethal dose. The medium was made a floating one in order to prevent the pill sinking into the mud at the bottom of a pool. The pills can be used for waste waters by trustworthy and well-trained agents only, but should be extremely cheap and effective. Health officers who desire to try them should write to the secretary of the school or to myself, when samples with directions for use will be forwarded.

Tables of *statistical error*, calculated for various degrees of probability, from 1 to 1 up to 99,999 to 1, will shortly be published in the *Annals*

of Tropical Medicine for the use of sanitary and medical workers by Mr Walter Stott, Honorary Statistician to the Liverpool School of Tropical Medicine, and myself.

With reference to section 21, Rirt and Boudet (*Soc. Med. des Hôpitaux*, 29th November 1907) publish a case in which a malarial relapse, verified microscopically, occurred in an old lady of seventy-one, who had been infected sixty years previously in childhood and who had lived in Paris for the last twenty years, certainly without reinfection.

D. Smith (*Proc. Canal Zone Med. Assoc.*, 1908, p. 113) reports finding that larvae of *Aedes perturbans* buries itself five inches in mud at the bottom of pools, deriving its air from the "root grass" to which it attaches itself.

Sir David Bruce describes (*Jour. Roy. Army Med. Corps*, April 1911) an inspection of Ismailia which he had just carried out for the purpose of studying the antimalarial measures there. Those who have felt any doubt as to the results of those measures should read the article.

(11). *Malaria Prevention in India.*—Several reports of local campaigns, showing that whatever the policy of the Government of India may have been, local officers are often doing their utmost against the disease, have been published. Among these I note only a few which I have been able to study. Captain A. B. Smallman, R.A.M.C. (*Jour. Roy. Army Med. Corps*, May 1911), well discusses prevention by treatment. He judges that out of 275 men who were given 10—15 grains of quinine (0·66—1·00 grammes) daily for four months, as advised by myself, 11 or 4% suffered from true recurrences (and not reinfections) in spite of the treatment (at Quetta). He finds that crescents take from nine to twenty days to disappear, uses enumerative methods, and estimates the number ingested by mosquitos as being possibly 12,000 (*vide* (4) above).

In the same number of the *Journal*, Colonel G. F. Gubbins, R.A.M.C., gives an excellent description of mosquito reduction among the military garrison of Bombay, with mention of difficulties, defects, cost, and results. He states that there was a great reduction in the numbers of all the species of mosquito since 1908, but cannot compute the effect on the malaria owing to changes of regiments and corps. The cost was 300—500 rupees *per annum* (£20—33)—much less, I believe, than the cost of invaliding a single soldier. Yet the Indian policy has been to declare that mosquito reduction is too expensive for practical use!

Captain M. F. Reaney, I.M.S., in a special official report (no publisher or date), has drawn up an excellent scheme for the town of Dinajpur. He says that a mosquito brigade was run for one year (1905–6) under Captain King, but was not continued after he left the station—another example of the discontinuity of such efforts in India. It remains to be seen whether Captain Reaney's scheme will have a happier fate.

Major J. C. Robertson, I.M.S., has given two most instructive reports on malaria in the towns of Nagina and Saharanpur, with maps and charts. Both towns are surrounded by wet cultivation; and he shows by a careful study both of death rates and spleen rates, that malaria diminishes rapidly from the outskirts of the towns to the centres, where it is least. Dinajpur had 21,412 inhabitants, and Saharanpur 66,254; and I estimate from the maps that the radius of the former is about 800 yards, and of the latter about 1,300 yards. He gives the diminution of the spleen rate in the former as being from 89% on the outskirts to 67% in the centre; and in the latter, from 75—90% on the outskirts to 61% in the centre. Actuals are unfortunately omitted; but the results are closely parallel to the death rates, and may therefore, I think, be taken as good evidence in favour of the law discussed in section 30 (8). Owing to the surrounding wet cultivation the disease is practically perennial. The author, like Colonel King, wisely recommends the compulsory change of the suburban cultivation from wet to dry.

For many years past malaria prevention in India has centred round the so-called official experiment at *Mian Mir*, briefly described in section 59. In October 1909, at the Malaria Conference at Simla, and probably in consequence of my own criticisms, a committee was appointed to report on the affair. This committee, consisting of two laymen and a medical man, spent only five days in the place, and completed their report a month later. The document did not reach me until the autumn of 1910, when it was criticised in the *Lancet* of the 5th November and the 3rd December 1910, by myself and Colonel W. G. King, respectively. Since then I have presented an exhaustive analysis of the whole subject to the India Office, to which Colonel King has added a full memorandum on malaria prevention in India.

The committee's report gives many details which, together with facts available from other sources, enable us to form a clear opinion of the affair. As stated in section 59, the experiment was commenced

in April 1902, but gave no results which could be accepted by scientific men as being of any real value. Since 1904 the military authorities undertook much more thorough work; but, to judge by the committee's report, this was gradually allowed to fall off owing to the smallness of the funds allowed, until, according to Colonel Rowan [1908], the *current* expenditure on the work was only about 1,000 rupees (£66) per annum—and this for Anopheline reduction over eight square miles occupied by sixteen thousand people! The cost amounts to 1 anna (about one penny) a head of population, and 1·7% of the annual income of the cantonment. The cantonment's funds were not assisted by the Imperial Government; no accurate records were kept, either of the cost or of the result of the operations; and yet the officials of the Indian Government have not scrupled to maintain that this was a scientific experiment carried out for the information of the whole world as to the practical utility of a new measure (mosquito reduction), designed for diminishing a disease which in India alone causes 1,000,000 deaths a year (by official estimate)! It would be difficult to refute me if I were to maintain that the whole affair was a public hoax arranged to save the authorities the cost and trouble of reducing malaria by the pretence that the suggested method had been tried and found to be impracticable.

Nevertheless, marvellous to relate, the chart of statistics given by the committee actually shows a great fall in the malaria rate, especially during the active campaign of 1905-6. The committee attempt to parry this by showing that a similar fall occurred at Amritsar, where, they say, "antimalarial measures have not been adopted on a large scale." Unfortunately for them, I have found the following passage in the *Annual Report of the Sanitary Commissioners with the Government of India*, 1908, Appendix, page 24: "*Amritsar.* The Cantonment Committee remark that the ponds and hollows in the Cantonment are being filled up as funds are available. . . . They say that the Cantonment suffers from shortness of funds, but still in ordinary years there is very little malaria that could be attributed to any conditions existing in the Cantonment." The fact is that even the trifling sum recently expended at Mian Mir probably had a very marked effect on the malaria rate.

For the further details I must refer the reader to the letters in the *Lancet* just mentioned. The committee attempted to measure the mosquitos and malaria by the usual random sampling, in which they ignore the statistical error. They do not discuss previous criticisms.

They give wholly misleading accounts of campaigns elsewhere, and, in fact, show no adequate knowledge of the subject. While thinking that the few rupees spent at Mian Mir constitute an enormous effort, they forget altogether to consider the cost of the disease there, which must, I think, amount to as much in one year as the antimalaria campaign has cost since it was commenced. But they are kind enough to conclude that "anti-larval measures combined with quinine prophylaxis offer great possibilities." Except for the action of General Kitchener in 1905-6 and of the military medical officers, the whole affair has been, I think, a disgrace to Indian administration.

Colonel King's memorandum just referred to will, I hope, be published before long. It is the work of a life-long student and director of Indian sanitation. He discusses the whole economical side of the subject in full, shows the immense cost of the disease, and the wisdom and pecuniary feasibility of adopting fundamental measures. It may not be generally known, but even at the present day India does not possess anything that can be called a sanitary service. Funds which should be expended in maintaining the health of the people are largely wasted in vain things, such as babu-education. Certainly most people like their children to be educated; but they also wish them to live. On comparing the expenditure on education and sanitation, however, one is driven to the conclusion that the Indian authorities prefer the former. Mian Mir has been the Sedan of the present sanitary administration of the country, and the system evidently requires complete reorganisation.

(12). *Some Historical Points.*—The remarks on India contained in section 57 have evidently stimulated those touched by them to several anonymous "reviews." A reply of mine to one of these has led to a furious rejoinder in which the anonymous author, evidently unable to answer my counter-criticisms, resorts to the usual personal abuse by pretending that I have not sufficiently acknowledged in Chapter I the assistance rendered to me in 1895-9 by Dr (now Sir) Patrick Manson (*Indian Medical Gazette*, April 1911). Chapter I is necessarily only an abstract of the long history of malaria; and my acknowledgments of assistance rendered to me by Manson, Laveran, and many others have been published by me over and over again, especially and in full detail in my lecture [1905]. For instance, in my Proteosoma Report [May 1898] I wrote: "These observations prove the mosquito theory of malaria as expounded by Dr Patrick Manson; and in conclusion I should add that I have constantly received the benefit of his advice

during the enquiry. His brilliant induction so accurately indicated the true line of research that it has been my part merely to follow its direction." And I thanked Dr Laveran elsewhere in a similar and suitable manner.

To-day, thirteen years after I wrote these words, I endorse every one of them. Since then I have read many scientific papers by juniors who have been assisted by the advice and views of their seniors, as, in fact, *always* happens; but, if I may say so, I have never read elsewhere any acknowledgment of such assistance more complete, or indeed so complete. The anonymous and ingenious reviewer of the *Indian Medical Gazette*, however, now uses this very passage of mine, and others like it, to maintain (*a*) that *all* my work was due to Manson, and (*b*) that because I have not said so in this book, and have not dedicated it to Manson (*sic*) I have "violated" in it "the unwritten laws of honour and of truth." I must therefore now describe again the exact extent of the assistance which he gave me.

Prior to 1894 I failed in finding the parasites of malaria in man, as I have often stated and as many others failed. This was due to the facts that we could not obtain the recent descriptive Italian literature in India, and because we had been misled by several writers who had been describing artifacts instead of the true parasites. In 1904, however, Manson showed me the latter, as, I believe, others had shown them to him. This is a service (always acknowledged by me in the proper place) which every senior renders as a matter of course to every junior; but it does not give the former a claim to the credit of future researches made by the latter. I have in my turn demonstrated the parasites to many juniors; but if any one were to make such a claim for me, I should repudiate it publicly at once as a matter of honour. In addition to this service, Manson occasionally supplied me with literature, and also urged government to place me on special duty. I received similar help from others, and the Government of Madras and the Maharajah of Patiala also offered to place me on special duty. It does not detract from the thanks due to Manson to observe that such services also are always, let us hope, rendered by seniors to juniors without constituting a claim to researches made by the latter; and I believe that Sir Patrick Manson himself would be the first to repudiate such a ridiculous suggestion.

Now as to the actual conduct of my Indian researches of 1895–8. As I have stated over and over again, they were based on Manson's induction of 1904, and also on Laveran's mosquito hypothesis of nine

years before this. That is to say, I inferred as a working hypothesis (*a*) from Laveran that mosquitos might be connected with malaria, and (*b*) from Manson, more exactly, that the so-called flagella very probably infect the insects. But the anonymous and ingenious reviewer is not content with this, and exclaims, "May I ask Major Ross why he neglected to indicate that those researches were carried out entirely on the instructions delivered to him personally by Sir Patrick?" For the simple reason that the statement is "entirely" untrue. When I gave thanks for advice tendered to me, I did not say that I always or even usually followed that advice. When I said that I had nothing to do but to follow Manson's induction, I did not say that I followed it by his methods and according to his instructions. I did *not* do so. I received from him and his induction merely the general indication to feed mosquitos on patients, and then to try to follow the so-called flagellated spores into the insect's tissues; but, as already explained in section 5, and at full length in my lecture [1905], this method was abandoned after a few months, because it was found impracticable. My methods (which were *not* advised by Manson) were (*a*) to keep the fed insects for several days and then to search them for any exceptional parasites which may have developed in them; (*b*) to try to infect persons by drinking water in which fed mosquitos had been kept; and (*c*) to try to infect persons by the bites of the insects. It was fortunate that I made the change so early, because Manson's method would have led only to failure, owing to the fact that the so-called flagellated spores were not such at all, but were sperms; and it was my method (*a*) which finally gave success. All this has been clearly set forth in my earliest writings, as the anonymous and mendacious reviewer, who has studied them with all the assiduity of personal animus, well knows. It may be thought that I had received full instructions regarding technique. This also is untrue. I learnt so little regarding mosquitos that I did not even know the zoological name of the group to which they belong till 1898; and when I commenced work in 1905, I was told that they live for only five days—*vide* Manson [1894 and 1896]; and it was I myself who found out how to keep them alive for longer periods. I obtained no literature or instructions, much less any demonstrations, on the methods of dissection, the anatomy, the feeding, keeping, the bionomics, and the classification and zoological characters of the insects—all of which I had to teach myself as best I could. The breeding and rough grouping of them I commenced to study years previously. To the present day

I do not know the technique employed by Manson in his work on *Filaria bancrofti* in mosquitos in 1877; and of course my technique was necessarily far more delicate. Moreover, it is not exact to say (as has been said) that my work was published by Manson. Owing to official delay, I was obliged to ask him to bring out preliminary notices of it; but the full details were published by myself. Lastly, I may point out that any one could have done in England what I was doing in India—the patients and the mosquitos were present, and the parasites have been easily cultivated in mosquitos in Hamburg and Holland. While therefore I heartily endorse to-day what I said on the subject thirteen years ago, I think that the acknowledgments which I made then and subsequently are sufficient; and I am not prepared to admit that my work was done by any one but myself. I may add that this book is dedicated to all genuine workers on malaria, among whom Manson is, of course, one of the most distinguished.

66. Theory of Happenings. (1). *Previous Work.*—The mathematical treatment adopted in section 28 has been met with some questioning by critics. Some have approved of it, but others think that it is scarcely feasible owing to the large number of variables which must be considered. As a matter of fact all epidemiology, concerned as it is with the variation of disease from time to time or from place to place, *must* be considered mathematically, however many variables are implicated, if it is to be considered scientifically at all. To say that a disease depends upon certain factors is not to say much, until we can also form an estimate as to how largely each factor influences the whole result. And the mathematical method of treatment is really nothing but the application of careful reasoning to the problems at issue. I propose, therefore, in this section, to give the calculations of section 28 a fuller treatment, in a manner which will be useful for students not only of malaria, but of other diseases, and in fact of other lines of work; and am convinced that many readers will be able to follow the work without difficulty.

The application of mathematics to epidemiology and even pathology has suggested itself to me (and perhaps to others) for many years; and when I came to write my Mauritius Report [1908], I found myself driven to make some attempt in this direction. I suggested there the name *Pathometry*, which can be applied to all mathematical study of infectious disease whether in the individual or in the community, and attempted the first applications to the time-to-time variation of malaria.

The principal difficulty lay in the careful consideration required to separate the numerous variables and to bring them into working shape; and this was effected by putting them into the form of a continued product of ratios, *bsbaimp*. From this the elementary difference equation of section 28 (4) was deduced. This is one of the first order and degree. I obtained (but did not publish) its solution (by my Verb-Functions) in the form of a series which when *m* is small reduces to a simple exponential function [1908, p. 35]. I gave also the static formula of section 28 (7), showed that the Anopheline factor must be large enough if the malaria ratio is to continue, and worked out several examples; but the report was written too hurriedly for further work. The subject was taken up again for this book at the end of 1909 with the results given in sections 28, 30, 30 (15), etc.—leading to the following laws:—

(*a*) The disease will not spread if the constants are too low.

(*b*) It will tend to reach a fixed limit depending on those constants.

(*c*) A small change in the constants (*e.g.* the Anopheline factor) may produce a great change in the malaria (section 30 (15)).

Nevertheless, as explained at the end of section 28, many variables, such as the effect of repeated bitings, were not considered.

Meantime I had asked Professor Karl Pearson for assistance, and he very kindly requested Mr H. Waite, M.A., B.Sc., to study the subject. I am extremely indebted to these gentlemen for their valuable help, which will give the reader more confidence in the results obtained. Unfortunately Mr Waite was not able to complete his studies until this book was published; and his article appeared in *Biometrika*, vol. vii. No. 4, October 1910. He had thus seen my Mauritius Report [1908], but not this book; and it will therefore be interesting to compare our results.

(2). *Mr Waite's Article.*—In my work I have hitherto taken the month as the unit of time during which the numbers of infected persons and of infective mosquitos are supposed to remain constant. That is, I supposed that at the beginning of the month there were *mp* infected persons and *sbaimp* mosquitos with protospores in their glands, ready to infect fresh persons. During the whole month these would deliver *bsbaimp* infective bites; but only a proportion of these would be inflicted on healthy persons, so that the increase in the number of infected

persons during the month would be $b^2saimp\,(1-m)$. But at the same time, I estimated that 0·2063 of the human cases existing at the beginning of the month would recover by the end of it; and I thus obtained equation (4) section 28, which shows the result of the gain and the loss during the entire month. But of course this is a considerable interval of time, and the truth would be that the number of infective mosquitos, of recovered persons, and of infected persons would be varying slightly from day to day. On the other hand, the number of infective mosquitos found on a given day does not depend, as suggested above, on the number of infective persons found on the same day, but on the number found from one to, say, three weeks previously; and also the newly infected persons would not be able to influence the equation by infecting fresh mosquitos until from two to three weeks—to allow for the incubation period and the production of gametids. On the whole therefore—I regret I omitted to explain this—I thought that the one-month unit of time would be best because it would, roughly speaking, cover the periods just mentioned. Moreover, unless the malaria rate was varying with perhaps unnatural suddenness, the number of infected mosquitos and persons would not change much during the month. In fact, we shall reach the same result if we suppose that all the events—the bites and the recoveries—are concentrated into one day in each month and do not occur again until the same day next month. And this position must be approximated to more and more as the disease approaches the static condition, when time-to-time variations are taken as ceasing.

Mr Waite's very interesting article is simply written, contains diagrams of curves and a number of carefully calculated examples, and will do much to convince the reader of the utility of such work. Referring to my section 28, he says: "From a comparison of the two accounts [his and mine] it will be seen that there is complete agreement between us on the essential and fundamental points, but some difference in numerical details. This difference is due to an attempt on my part to treat the question by fuller and more rigid mathematical methods, which would be out of place in a treatise written from the medical standpoint, while the simpler methods employed by Professor Ross give results which are sufficiently accurate for practical purposes."

The difference in numerical details is, as he explains, due to his having taken for a time unit, not the month selected by me, but the interval "between two consecutive infecting bites." He adopts my continued product of ratios, $bsbaimp$ (with some small alterations

which it is unnecessary to repeat here), and writes $n = b^2saimp$. Hence n is the number of infected bites delivered (as before) during an entire month. But he now divides the month into n successive intervals, each of which he takes as his unit of time; and then estimates the recovery rate by my standard, not for the entire month as I did, but for the nth part of a month; and so forms his difference equation.[1]

As he rightly points out, this shorter unit of time enables us to deal more continuously both with the increase of malaria due to the bites, and with the decrease due to the recoveries; but there is a third variable, namely, the number of infective mosquitos, n, which, in its turn, depends on the malaria rate of a few weeks previous. Would it not therefore be more correct to employ the same unit of time for all the variables together? By keeping n constant while m and rm vary, both the increase and the decrease of the malaria rate are rendered too slow. Thus the changes shown by Mr Waite's figures are not so rapid as mine (just as he says); but I am not clear that they approximate nearer to the truth.

For static malaria Mr Waite obtains the following equation (put in the terms of section 27):

$$a = \frac{c}{1 - m} - \frac{c}{2mp} - \frac{c(1 - m)}{12m^2p^2}$$

This ceases to hold when $amp < 192$; that is, when there is less than one inoculation in the month (see end of section 27), in which case the results are indeterminate. The constant $c = 44{\cdot}36$, and is a little higher than my lowest malaria-producing limit of Anophelines, namely, 39·60. The form of my equation for static malaria (section 28 (7)) is evidently the same, allowing for the difference in the constant, as the first term of the above, which is obviously the most effective term.

The following table shows the relation as given by him (page 426) between the number of malaria cases (mp) and the number of Anophelines per man (a) for static malaria in a population of 1,000.

$a =$	40	44	46	49	52	55	59	63
$mp =$	5	20	50	100	150	200	250	300

Observe the great rise in the malaria rate compared with the small one of the Anophelines. The latter could scarcely be detected by our present methods—as already pointed out in section 30 (15), etc.; the former would be a severe epidemic.

[1] See (9) below.

(3). *The Problem of Happenings and Becomings.*—But it is always advisable to commence such studies from the widest possible point of view. I will therefore now discuss a much more general theorem, which contains the malaria problem as a particular case, and which, though still incomplete, may be of use to the sanitarian in many ways. We shall deal with time-to-time variations not only of malaria, but of all disease, and not only of diseases of man, but those of any living organisms. Still further, as infection is only one of many kinds of events which may happen to such organisms, we shall deal with *happenings* in general.

Problem I.—Consider a population of any organisms living in an area, and suppose that in unit of time some event happens to a constant proportion of them. Suppose also that the population is being perpetually changed by a constant birth-rate, death-rate, immigration rate and emigration rate. Then, at the end of a given period, what proportion of the population found living in the area will be also found to have been affected by the happening; and to what proportions will the event have happened once, twice, thrice, etc.?

The happenings may be some kind of accident or disease, birth, death, marriage, or anything else that we can think of—vaccination, receipt of bequests, conversion to some creed, etc. But we now observe that they may be of two kinds, those which occur once for all, and those which affect the subject for some given period, after which they cease to affect him. In the latter case we have to deal with *becomings* or *conversions* rather than simple happenings; and this group contains diseases from which recovery and loss of immunity take place—which leads to another problem.

Problem II.—Suppose that in addition to the data of Problem I a constant proportion of the affected class revert to the unaffected class in unit of time, how will the proportions then stand at the end of the given period?

Both problems can be solved together as follows:

(4). *Solution of the Equations.*—Let p_t be the total population at the end of the period t. Let a_t be the proportion of them to which the happening has not occurred at all; b_t be the proportion to which it has occurred only once; c_t the proportion to which it has occurred only twice, and so on. And let z_t be the whole number of those to which the event has occurred.

To begin with we shall consider only a_t and z_t, that is the total *unaffected* and the total *affected* individuals. And obviously,

$$p_t = a_t + z_t.$$

Let v be the variation in unit of time due to births, deaths, immigration and emigration among the *unaffected*, a_t; and V be the similar variation among the *affected*, z_t. Let ha_t be the proportion of the unaffected which become affected in unit of time, and Hz_t be the proportion of the affected which become unaffected (*i.e.* which revert or recover) in unit of time. And for brevity let $h' = 1 - h$, and $H' = 1 - H$. Then,

$$\begin{aligned} a_{t+1} &= h'v.a_t + HV.z_t \\ z_{t+1} &= hv.a_t + H'V.z_t \\ p_{t+1} &= v.a_t + V.z_t \qquad . \quad . \quad . \quad . \quad . \quad (a) \end{aligned}$$

These difference equations are obviously true because the gain of one group is the loss of the other group, and the total population is composed of the sum of the two groups without the factors h and H. We proceed to solve them without further discussion at the moment. Eliminating z_t from the first two equations we have,

$$H'V.a_{t+1} - HV.z_{t+1} = (h'v.H'V - hv.HV)a_t$$

Hence $\quad H'V.a_{t+1} - (a_{t+2} - h'v.a_{t+1}) = (h'H' - hH)vV.a_t$

That is $\quad a_{t+2} - (h'v + H'V)a_{t+1} + (h'H' - hH)vV.a_t = 0$

Similarly $\quad z_{t+2} - (h'v + H'V)z_{t+1} + (h'H' - hH)vV.z_t = 0 \qquad . \quad . \quad (b)$

The solution of this difference equation is composed of the roots of the ordinary algebraic quadratic

$$x^2 - (h'v + H'V)x + (h'H' - hH)vV = 0 \qquad . \quad . \quad . \quad (c)$$

Let X and Y be those roots. Then

$$\begin{aligned} a_t &= cX^t + CY^t \\ z_t &= c'X^t + C'Y^t \qquad . \quad . \quad . \quad . \quad (d) \end{aligned}$$

where c, C, c', C' are constants to be now determined. Putting successively $t = 0$ and $t = 1$ in the equations (d), we have

$$\begin{aligned} a_0 &= c \;\; + C \\ a_1 &= cX + CY \\ z_0 &= c' \;\; + C' \\ z_1 &= c'X + C'Y \end{aligned}$$

From these, c, C, c', C' can easily be obtained, and we have finally

$$(X-Y)a_t=(a_1-a_0Y)X^t-(a_1-a_0X)Y^t$$
$$(X-Y)z_t=(z_1-z_0Y)X^t-(z_1-z_0X)Y^t$$
$$(X-Y)p_t=(p_1-p_0Y)X^t-(p_1-p_0X)Y^t \quad . \quad . \quad (e)$$

where

$$a_1=h'v.a_0+HV.z_0$$
$$z_1=hv.a_0+H'V.z_0$$
$$p_1=v.a_0+V.z_0$$
$$p_0=a_0+z_0$$

The algebraic quadratic (c) is a perfect square if $hHvV=0$; in which case we have

$$X=h'v \quad Y=H'V \quad . \quad . \quad . \quad . \quad (f)$$

The quadratic may also be written in the form,

$$x^2-\{v+(h'-H)V-h(v-V)\}x+v.(h'-H)V=0 \quad . \quad (g)$$

This also is a square if $h(v-V)=0$, or is small enough to be neglected —a thing which occurs when v and V are the same or nearly the same, or the unit of time taken is small. In this important case

$$X=v \quad Y=(h'-H)V$$

Otherwise the roots of the quadratic may be irrational. They will always be of the form $X=P+Q$, $Y=P-Q$, where

$$4P^2=(h'v+H'V)^2$$
$$4Q^2=(h'v+H'V)^2-4v.(h'-H)V \quad . \quad . \quad (h)$$

so that

$$X-Y=2Q$$

and $$XY=v.(h'-H)V$$

Hence, since $$YX^t-XY^t=XY(X^{t-1}-Y^{t-1})$$

the equations (e) may be written

$$(X-Y)a_t=a_1.(X^t-Y^t)-a_0.v(H'-h)V(X^{t-1}-Y^{t-1}) \quad . \quad (i)$$

etc., etc., on expanding which the radicle disappears.

(5). *The Variation-Constants.*—This solution holds only when v, V, h, H are constants for the period under study. We have now to discuss these constants more exactly, beginning with v and V.

Let n, m, i, e denote respectively the nativity, mortality, immigration and emigration ratios in unit of time among the *unaffected*; and

N, M, I, E the similar ratios among the *affected*. Then it is best to write

$$v=(1+n)(1-m)(1+i)(1-e)$$
$$=n'm'i'e' \text{ (say)}$$
$$V=(1+N)(1-M)(1+I)(1-E)$$
$$=N'M'I'E' \text{ (say)} \quad . \quad . \quad . \quad . \quad . \quad (j)$$

Here the symbols on the right denote positive numbers, the magnitude of which depend on the magnitude of the unit of time for which they are taken. If that unit is one year, for instance, and the annual nativity is 25 per mille, and the annual mortality is 20 per mille, we should have to put $n=\cdot 025$, $m=\cdot 02$. But if we reduce the unit to a month, a day, an hour or less, we must also reduce the values of n and m; and the same observation applies to the other constants; while v and V then approximate to unity.

If the unit of time, or the constants themselves, are very small, their products may be neglected, and we have

$$V=1+n-m+i-e$$
$$v=1+N-M+I-E \quad . \quad . \quad . \quad . \quad (k)$$

Or we may proceed as follows. We have

$$v^t=(1+n)^t(1-m)^t(1+i)^t(1-e)$$

Now suppose that all the symbols refer here to an infinitesimally small unit of time, and suppose that t is the number (extremely large) of such small units contained in some finite unit such as one day. Then each of the factors takes the well-known exponential form, and we have

$$v=\epsilon^{n-m+i-e} \quad . \quad . \quad . \quad . \quad . \quad (l)$$

with a similar expression for V. *Now*, of course, the symbols refer, not to the infinitesimal unit, but to the finite one. The first two terms of the expansion of the exponential gives the same result as (k) above.

It is necessary to assume that each of the variation factors *may* be different for the two groups a and z. For example, the mortality and emigration may be much greater, and the nativity and immigration less, in certain diseases. I have therefore used different symbols for v and V, and for their constituents. Also m, M, e, E are always not greater than unity.

(6). *The Happening and Reversion Constants*.—Consider now the following equations.

$$\begin{aligned}
a_{t+1}&=(1-h)a_t+(1-h)na_t+(1-h)Nz_t+(1-h)rz_t\\
z_{t+1}&=\quad ha_t+\quad hna_t+\quad hNz_t+\quad hrz_t+(1-r)z^t\\
p_{t+1}&=\quad a_t+\quad na_t+\quad Nz_t+\quad z_t \quad . \quad . \quad . \quad (m)
\end{aligned}$$

These are evidently the same as equations (a) above; but in greater detail and without the mortality, immigration and emigration ratios. Looking first at the equation for p we see that it is the sum of the others, and that p_{t+1} consists of a_t and z_t *plus* the births that have occurred to the two groups in the interval of time between t and $(t+1)$—just as it should do. The factor h denotes the ratio of individuals to which the happening has occurred in the same unit of time, and the factor $(1-h)$ those to which it has not occurred. Thus ha_t is the proportion of the *unaffected* group which pass over to the *affected* group in the unit of time, and $(1-h)a_t$ the proportion which remain behind. The second and third terms of the equations denote a similar change among the offspring derived from the two groups (na_t and Nz_t)—it being assumed that the offspring of the affected are not themselves affected at birth, though some may become so immediately afterwards.[1] The symbol r denotes the proportion of the affected group which revert to the other group in the unit of time, as by recovery from disease; and the factor $(1-r)$ those that do not revert. Since the happening may occur again to some of those that revert immediately after reversion (as it may occur to the new-born immediately after birth) it is necessary to include the term hrz_t; but this and the terms hna_t and hNz_t will generally be small. The equations therefore seem to include every possibility. They may be written

$$a_{t+1} = (1-h)(1+n)a_t + (1-h)(N+r)z_t$$
$$z_{t+1} = h(1+n)a_t + \{h(N+r) + (1-r)\}z_t$$

But
$$N + r = \left(1 - \frac{1-r}{1+N}\right)(1+N)$$

and
$$h(N+r) + (1-r) = \left\{h + (1-h)\frac{1-r}{1+N}\right\}(1+N)$$

Hence, putting $R = \frac{1-r}{1+N}$, $h' = 1-h$, $n' = 1+n$, $N' = 1+N$, we have

$$a_{t+1} = h'n'a_t + (h' - h'R)N'z_t$$
$$z_{t+1} = hn'a_t + (h + h'R)N'z_t$$

Lastly, to bring in the mortality, immigration and emigration factors, as

[1] I think that this is the better method. But it would be easy to add a special term for *affected births*.

shown in equation (j), we have only to multiply a_t by $m'i'e'$ and z_t by $M'I'E'$; and we get

$$\begin{aligned} a_{t+1} &= h'n'm'i'e'.a_t + (h' - h'R)N'M'I'E'.z_t \\ &= h'v.a_t + HV.z_t \\ z_{t+1} &= hn'm'i'e'.a_t + (h + h'R)N'M'I'E'.z_t \\ &= hv.a_t + H'V.z_t \end{aligned}$$

as in equations (a), which have already been solved. Thus

$$\begin{aligned} H &= h' - h'R \\ H' &= h + h'R \end{aligned}$$

Also, as $h' - H = h'R$, the radicle in the roots of the quadratic equation (c) may be written

$$4Q^2 = \{v + h'RV - h(v - V)\}^2 - 4v.h'RV$$

and we obtain the following values of $2Q$, X and Y for the study of particular cases.

if $v = V$	$X - Y = 2Q = v - h'RV$	$X = v$	$Y = h'RV$	
,, $h = 0$	,, $= v - RV$	,, $= v$	,, $= RV$	
,, $h = 1$	,, $= V$	,, $= V$	,, $= 0$	
,, $V = 0$	,, $= h'v$	,, $= h'V$	,, $= 0$	
,, $R = 0$	,, $= h'v + hV$	,, $= h'v + hV$	,, $= 0$	
,, $R = 1$	,, $= h'v - V$	,, $= h'v$	,, $= V$	. (n)

(7). *The Equivariant Case.*—We have thus obtained integral expressions for a_t, z_t and p_t in terms of t, and of the ten independent constants h, r, n, m, i, e, N, M, I, and E; and the finite form in which they have been obtained has this advantage over the infinitesimal form, that it enables us to employ any unit of time we please—as may sometimes be required for particular cases. The reader can now study some such cases—accidents, diseases, etc.; and it will be seen that each disease will have its special equation. I must, however, confine myself to the particular case of equation (g), when $v = V$, that is, when the kind of happening under consideration does not greatly modify the birth- and death-rates of the affected group. This may be accepted for the present in the case of malaria, especially since the equation $v = V$ does not imply that each factor in v equals each factor in V. The case includes also most of the milder kinds of disease; and the formulae will be useful for a number of calculations required for public health.

(i). From equation (g) if $v=V$, $X=v$ and $Y=(h'-H)V$; and equations (e) then become

$$(h+H)a_t=(Ha_0+Hz_0)v^t+(ha_0-Hz_0)(h'-H)^tV^t$$
$$(h+H)z_t=(ha_0+hz_0)v^t-(ha_0-Hz_0)(h'-H)^tV^t$$
$$Pt=P_0v^t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (o)$$

The constants in terms of the elements are

$$H=(1-h)(N+r)/(1+N)=h'(N'-r')/N'$$
$$h+H=(N+h+r-hr)/(1+N)=1-h'r'/N'$$
$$h'+H=(1-h)(1-r)/(1+N)=h'R=h'r'/N'$$

By substituting p_t for p_0v^t we have the more symmetrical forms,

$$\frac{a_t-Y^ta_0}{H}=\frac{p_t-Y^tp_0}{H+h}=\frac{z_t-Y^tz_0}{h}$$

$$\frac{hp_t-(h+H)z_t}{hp_0-(h+H)z_0}=\frac{Hp_t-(h+H)a_t}{Hp_0-(h+H)a_0}=\frac{ha_t-Hz_t}{ha_0-Hz_0}=Y^t$$

Useful forms are

$$a_t-Y^ta_0=\frac{(1-h)(N+r)}{N+h+r-hr}(p_t-Y^tp_0)$$

$$z_t-Y^tz_0=\frac{h(1+N)}{N+h+r-hr}(p_t-Y^tp_0)$$

and if $v=V$ exactly,

$$z_t-(h'R)^tz_0.p_t/p_0=\frac{h}{1-h'R}\{1-(h'R)^t\}p_t \quad . \quad . \quad (p)$$

Obviously a_0 and z_0 are the numbers of the *unaffected* and the *affected* groups at the beginning of the enquiry, and when $z_0=0$, and therefore $a_0=p_0$, we suppose that the enquiry begins at the same time as the happenings. When v^t and $V^t=1$, the population remains constant in number.

(ii). As stated in (5) above, if the unit of time taken is very small, the elements are also very small and their products may be neglected. Thus $\frac{(1-h)(1-r)}{1+N}$ becomes $(1-h-r-N)$; and if $z_0=0$, we have

$$z_t=\frac{h}{N+h+r}\{1-(1-N-h-r)^t\}p_t$$

$$=h\left\{t-\frac{t(t-1)}{1.2}(N+h+r)+\text{etc.}\right\}p_t \quad . \quad . \quad . \quad (q)$$

which is useful for quick calculation when t is not too large. If the

unit of time is infinitesimally small, v^t and Y^t can be put in the exponential form of equation (l), and we have

$$z_t = \frac{h}{N+h+r}\left\{1-\epsilon^{-(N+h+r)t}\right\}\epsilon^{(n-m+i-e)t}p_0 \quad . \quad . \quad (r)$$

See also (14) below.

As $h'R$ or $\dfrac{(1-h)(1-r)}{1+N}$ is less than unity, $(h'R)^t$, or $\epsilon^{-(N+h+r)t}$, decreases without limit as t increases; so that, if the elements remain constant and t is large enough, we have ultimately,

$$z_t = \frac{h(1+N)}{N+h+r-hr}p_t \quad . \quad . \quad . \quad . \quad . \quad (s)$$

That is, z_t reaches a fixed limit. But if the elements are small, even when referred to a finite unit of time, t may have to be very considerably large before this limit is nearly approached.

Although $v = V$, I have still retained both symbols where necessary, because this equivalence does not always denote that each element in v is equal to each element in V. For the same reason I have retained N. The asymmetrical position of this element in the equations is due to the fact that according to the postulate of (6) the offspring of the affected group are born unaffected, and are therefore not subject to recovery. On the other hand, the immigration element does not possess this asymmetrical position, because the immigrants are supposed to be affected in the same ratio as the considered population. Moreover, the equations (o) still hold approximately (equation g), even if v does not exactly equal V, if $h(v-V)$ is small enough to be neglected in comparison with $v+(h'-H)V$; and when the unit of time taken is small enough, the former expression tends to vanish while the latter approximates to 2.

(iii). The following are some special cases. If we retain h only, equations (o) and (p) become

$$a_t = (1-h)^t a_0 \qquad z_t = p_0 - (1-h)^t a_0$$

so that, if t is large enough, $a_t = 0$ and $z_t = p_0$; that is, as there are no reversions, the whole of the original population becomes affected. If we retain also all the other factors except n, N, and r, we have

$$a_t = (1-h)^t (M'I'E')^t a_0 \qquad z_t = (m'i'e')^t p_0 - a_t \quad . \quad . \quad (t)$$

(iv). If we retain only r,

$$a_t = p_0 - (1-r)^t z_0 \qquad z_t = (1-r)^t z_0 \quad . \quad . \quad . \quad . \quad (u)$$

The latter equation gives the decrease in an affected population by the

reversions alone in the absence of any fresh happenings—as already used in section 28 and in Mr Waite's article to calculate the recovery ratio. The mortality, immigration and emigration elements can be added as in (iii).

The reversion ratio may be a function of the happening ratio as in the case of divorce and marriage; but in disease it is generally independent, being a pathological element, while h is an epidemiological one. For example, in a disease in which 99% of the cases "recover" in 10 days and r is the daily recovery ratio, $r = 1 - \sqrt[10]{\cdot 01} = \cdot 369$. Yet the happening ratio may be large or small. See also (12, v) below for a definition of the term in connection with disease.

(v). Retaining only h and r, and if $z_0 = 0$,

$$z_t = \frac{h}{h(1-r)+r}\{1-(1-h)(1-r)^t\}p_0 \quad . \quad . \quad . \quad (v)$$

Thus if $r = \cdot 369$ *per diem* and $h = \cdot 01$ *per diem*, and $p_0 = 1000$, the number of affected people in each of the first five days would be about 8, 17, 21, 25, and 26, rising to 26·88 in ten days and to nearly 27·1 subsequently. If however $h = \cdot 1$, the value of z_t would rise to 99·977 in the first day, and would subsequently asymptote to 231.

It should be observed that, as long as h and the other elements *are constant*, the rise in the value of z_t is continuous, but is rapid at first and slower as it approaches the final limit.

The coefficient $h/(h+H)$ in the expression for z_t may be called the *modulus* of the happenings and be denoted by μ; so that $z_t = \mu p_t$ when t is large enough. This may be named the *static* value of the happening-equation.

(8). *Repeated Happenings.*—But in subsection (3) we asked "to what proportion of the population will the event have happened once, twice, thrice, etc.?" And in subsection (4) I adopted the following notation—a_t is the number of individuals to which the event has not happened at all; b_t, c_t, d_t . . . the numbers to which it has happened only once, twice, thrice, etc.; and z_t the whole number of those to which the event has occurred any number of times. We have obtained the values of a_t and z_t; it will be now useful in many ways to find those of b_t, c_t, d_t, etc. They can be obtained from the following difference equations:—

$$\begin{aligned} b_t &= hva_{t-1} + (1-h)Vb_{t-1} \\ c_t &= hVb_{t-1} + (1-h)Vc_{t-1} \\ d_t &= hVc_{t-1} + (1-h)Vd_{t-1} \quad . \quad . \quad . \quad (w) \end{aligned}$$

Obviously the number of unaffected individuals that become affected in unit of time, namely ha_{t-1} pass over into group b; while the number of this group which are unaffected, namely $(1-h)b_{t-1}$ remain in it. Precisely similarly hb_{t-1} individuals pass over from group b into group c, while $(1-h)c_{t-1}$ remains in the latter; and so on.

The solution of these equations is rather complex, if we retain the asymmetrical elements N and r; but easy if we retain only the mortality, immigration and emigration factors. In this case (noting that V holds only these elements), we have, if $v=V$,

$$\begin{aligned} a_t &= (1-h)^t V^t p_0 \\ b_t &= h(1-h)^{t-1} v^t p_0 + (1-h) V b_{t-1} \\ &= th(1-h)^{t-1} V^{t-1} p_0 \end{aligned}$$

Similarly
$$c_t = \frac{t(t-1)}{1.2} h^2 (1-h)^{t-2} V^t p^0$$

and
$$d_t = \frac{t(t-1)(t-2)}{1.2.3} h^3 (1-h)^{t-3} V^t p_0 \quad . \quad . \quad (x)$$

That is, all these quantities are merely the terms in the expansion of $\{(1-h)+h\}^t V^t p_0$. The last term, $h^t V^t p_0$, denotes the numbers to which the event has occurred the greatest possible number of times, namely t times. The sum of all the terms is $1^t V^t p_0$ or p_t, as it clearly must be; and $z_t = b_t + c_t + d_t \; . \; . \; . \; . = p_t - a_t$.

Moreover, as can easily be proved,

$$0.a_t + 1.b_t + 2.c_t + 3.d_t + \; . \; . \; . \; . = thp_t \quad . \quad . \quad (y)$$

that is, if we multiply each group by the number of times the event has happened to it, we obtain the whole number of events which have occurred since the beginning. Since h is the proportion of events which occur in unit of time, th is the proportion which occur in t units.

Now $\{(1-h)+h\}^t = (1-h)^t\left(1+\frac{h}{1-h}\right)^t$; and if we write h/t for h, and tx for t, as in equation (r), and let t increase without limit, this expression becomes $\epsilon^{-hx}(1+hx+\frac{1}{2}h^2x^2+\frac{1}{6}h^3x^3 \; . \; . \; .)$, which $=\epsilon^{-hx}\epsilon^{hx}$. That is, if we are considering a very large number of very small time units, the proportions of individuals to which the event occurs 0, 1, 2, 3 . . . times, are as the terms in the expansion of ϵ^{hx}. On multiplying each term by the number of times the event has happened to it, we have, as before, $hx\epsilon^{-hx}\epsilon^{hx} = hx$ for the whole number of events since the

beginning. Here h is the happening ratio for a finite unit of time, such as one day.

The average number of events which have occurred to each *affected* individual during the time t will be found by dividing the whole number of events by the whole number of those to which they have occurred (if the population remains the same). That is, it will be $th/\{1-(1-h)^t\}$.

(9). *Verification of Section* 28.—We can now proceed to apply this theory of happenings to our subject; but may first demonstrate a verification of equation (4) of section 28. In equations (0) above, put z_{t-1} and p_{t-1} for z_0 and p_0 (which can of course be done); and, remembering that the modulus $\mu = h/(h+H) = h/(X-Y)$ and that $p_t = Xp_{t-1}$, we have the simpler difference equation,

$$\begin{aligned} z_t &= Yz_{t-1} + \mu(p_t - Yp_{t-1}) \\ &= Yz_{t-1} + \mu(X-Y)p_{t-1} \\ &= (1-h)(1-r)z_{t-1} + hp_{t-1} \\ &= z_{t-1} + h(p_{t-1} - z_{t-1}) - (1-h)rz_{t-1} \quad . \quad . \quad (z) \end{aligned}$$

if we neglect the variation elements. This is the same as equation (4) of section 28, except that the small factor hr, denoting the happenings among the reversions, is retained, and the expression for the proportion of infected bites, b^2saim, is replaced by the constant fraction h. Putting $z_t = z_{t-1}$, we have

$$z_t = \mu p_t,$$

which, if we put b^2saiz_t/p_t for h, reduces to the static formula $z/p = 1 - r/b^2sai$ of equation (7) section 28. Mr Waite's equation is merely the particular case when we take $h = 1/p$.

Though in the case of disease-happenings, h must often, or usually, be a function of z_t, it was necessary to begin a general study of the subject with the conception of it as a constant, because there is no clear *a priori* reason for assigning the form of function referred to; but some indications will be obtained presently in the following cases.

(10). *Application to Static Metaxenous Diseases.*—From section 4 these are diseases due to parasites which are common to two (or more) species of animals. I say that a disease becomes static when z^t asymptotes to a fixed limit, if all the elements, except the time, remain constant. Obviously the same equations (with different elements) will apply to both species of animals. Let us use marked letters for the equations of one species (these will not be confounded with the marked

letters previously used—which are no longer required). Then the static equations for the two species will be

$$z = \mu p \quad z' = \mu' p'$$

(We could continue to use p_t here if we like). These may be written

$$h\{(1+N)p-(1-r)z\} = (N+r)z$$
$$h'\{(1+N')p'-(1-r')z'\} = (N'+r')z' \quad . \quad . \quad (aa)$$

Now h and h' represent the infection rates in the two species of animals: and the infection—that is, the parasite common to both species—passes from one species to the other and back again. Hence there must be a relation between h and z' and between h' and z. When $z' = o$ then also $h = o$, since no infected animals of the z' species are left from which the infection can be conveyed to the other species; and if z' is large we may expect (and, indeed, find) that h also is large. And precisely the same things may be said of the relation between h' and z. Hence we may assume, to begin with, that the relation is without a free constant and is of the first degree; and therefore put

$$h = k'z' \quad h' = kz$$

where k and k' will be discussed presently. Substituting these values for h and h' in equations (aa) and eliminating z' between them, we obtain

$$z\{(1-r)k(1+N')k'p' + (1-r')k'(N+r)\} =$$
$$(1+N)kp(1+N')k'p' - (N+r)(N'+r') \quad . \quad . \quad (ab)$$

with a similar equation for z'. From these, if the elements are known, the static numbers of affected animals of both kinds can be found.

(11). *Application to Certain Insect-borne Diseases.*—The factors k and k' will vary for different diseases. An important group of infections is that in which the parasites common to the two species of animals are communicated at both transferences in the few minutes during which one species of host feeds on the other species; and many diseases of animals and apparently all the mosquito-borne diseases of man—malaria, yellow fever, dengue, filariasis—belong to the group. That is to say, both h and h' occur during a very short time unit or element. To calculate k and k' for this group we proceed as follows: First take a very short time unit (say one minute); and suppose that bp is the number of persons bitten and $b'p'$ the number of mosquitos (or other animals) which bite during it. Then as the time unit is so short we may grant each insect has fed only once during it and has bitten only one person; and that, conversely, each person has been

bitten only once during it, and by only one insect. Hence the number of persons bitten during the time unit equals the number of biters; that is, $bp = b'p'$. As b' is a ratio which applies to all or any of the insects present it applies also to the infected ones; that is, $b'z'$ will be the number of *infected* mosquitos which bite during the short time-unit and will also be the number of persons that are bitten by such. But not all the *infected* mosquitos, z, will also be *infective*; let $f'z'$ be the number of the latter. Then $b'f'z'$ will be the number of infective mosquitos which bite during the time unit, and also the number of persons (healthy or already infected) which are bitten by such. During the same time mosquitos will become infected from men. Let z be the number of infected persons, and fz the number of these which are also infective to mosquitos. Then bfz will be the number of such which are bitten by mosquitos during the time unit, and also the number of mosquitos which bite them.

But h and h' are the proportions of persons and of mosquitos to which the infection-event happens in a time unit, and p and p' the two populations. Some of these persons may possibly be *naturally* immune—that is, may not become infected although they have never had the disease and recovered from it. Let g and g' be the proportions (probably nearly unity) which are *not* naturally immune. Then hgp and $h'g'p'$ will be the respective numbers to which, whether they are already infected or not, the infection-event occurs in the unit of time. Hence—

$$hgp = b'f'z' \qquad h'g'p' = bfz \quad . \quad . \quad . \quad (ac)$$

and since we wrote $h = k'z'$ and $h' = kz$, we have

$$k = \frac{b'f'}{gp} \qquad k' = \frac{bf}{g'p'}$$

Substituting these in equation (ab) we have

$$z.bf\{(1-r)(1+N')b'f'p' + (1-r')(N+r)gp\} =$$
$$pp'\{(1+N)(1+N')bfb'f' - (N+r)(N'+r')gg'\} \quad . \quad (ad)$$

with a similar equation for z', the disease among the insects. Note that also $bp = b'p'$. The above equation gives of course only the *static* value of z, in terms of all the elements concerned.

(12). *The Malaria Constants.*—It remains to calculate these elements as best we can; but this can be done at present only on very insufficient information, and often on little more than merely conjectural ground. The reader must be warned on this point; but he will see

also that small differences in the actual figures are of less importance than the form of the equations obtained.

(i). *Deathrate and Birthrate of Anophelines.*—As stated in Section 29 (2) we know little or nothing regarding the average length of life of Anophelines, even in captivity, and much less when free. If we accept the conjecture there made, that only one-third survive for ten days (long enough to allow the plasmodia to develop in them), we can calculate the *mortality ratio* for one day (and therefore for any period) from the formula $(1-m)^{10} = 1/3$; that is from

$$10 \log (1-m) = \log 1/3$$

Thus for one day $(1-m) = {}^{\cdot}8959586$ and $m = {}^{\cdot}1040414$; and for one minute $(1-m) = {}^{\cdot}9999234$ and $m = {}^{\cdot}0000766$. Obviously, at this rate 1/9th the Anophelines survive for 20 days and 1/27th for a month (30 days); and so on.

The birthrate presents similar difficulties, because the number of eggs laid by a female is no indication of the number which survive. But if we suppose that the mosquito population is remaining stationary we can obtain n from the formula $(1+n)(1-m) = 1$. Thus for one day $(1+n) = 1{\cdot}116125$ and $n = {}^{\cdot}116125$; and for one minute $(1+n) = 1{\cdot}0001016$ and $n = {}^{\cdot}0001016$.

The immigration and emigration ratios can be calculated from similar formulae if we know the amount of the migration at the end of any definite period. For instance, if one third of the mosquitos leave a given area by the end of ten days the rates per day or minute will of course be the same as the deathrate calculated above; and if the immigration and emigration are equal, the immigration rate will be the same as the birthrate calculated above. In section 29 (16) it was shown that, *ceteris paribus*, the migration ratio into or out of an area will vary inversely as the radius of the area, and reference was made to the law of random scatter in 29 (14); but unfortunately we possess no trustworthy observations on these subjects—which I cannot discuss here. We should observe that deaths and emigrations can be dealt with as ordinary happenings (the former not subject to r)—as for instance by 7 (iii).

(ii). *The Biting Factor.* Similarly the *bitings* of mosquitos can be taken as ordinary happenings in the life of the insects, and *being bitten* by them as such in the life of their victims; and both can therefore be calculated from the equations just referred to (not subject to recovery). Moreover, as argued in (11), if a small enough unit of time be taken,

the active and passive bitings will number the same—each insect may be considered as biting only one and a different person in the time. Take, therefore, one minute as the unit of time, and let B_1 and B'_1 be respectively the *number* of persons and of mosquitos bitten and biting during the minute; and let b and b' be the respective *proportions* of the two populations, p and p'. Then

$$bp = B_1 = B'_1 = b'p'$$

Now, if B_t and B'_t be the numbers of persons bitten and of mosquitos biting during the period t (stated in minutes), we can find their values by any of the happening equations (with $r = 0$) of (7). Thus putting B_t for z_t and b for h, and $B_o = 0$, we have

$$B_t = \frac{b(1+n)}{b+n}\left\{1 - \left(\frac{1-b}{1+n}\right)^t\right\} v^t p_o \quad . \quad . \quad . \quad (ae)$$

or any other form suitable for numerical calculation; with the same equation for B'_t. The simplest way to form concrete ideas on the subject is to assign a series of values to the minute-happenings b and b' and then to calculate B_t and B'_t for one day (1440 minutes). Suppose that b and b' are ·000001, ·00001, ·0001, ·001, and ·01 successively; let p and p' be 1,000,000 each; and let this number remain constant—that is, let v^t and $v'^t = 1$ during the period under study.

The equations now contain only b_1 n, b'_1 n'; and we have to calculate from them the number of persons bitten (B_{1440}) and of mosquitos biting (B'_{1440}) during one whole day. With regard to the former we may omit the birthrate n. The human birthrate (say 25/1000 per annum) will amount only to ·000,000,005 or less in a minute, as calculated by the method of (i) above, and will have small effect on the calculation for one day. But the mosquito birthrate (adults) will be much larger, namely ·0001016 by the conjectural standard of (i), and has considerable effect. The results are as follows:—

Human Population, 1,000,000

Bitten per minute .	1	10	100	1,000	10,000
Bitten at end of day .	1,439	14,297	134,119	763,243	999,999·5

Mosquito Population, 1,000,000

Fed per minute .	1	10	100	1,000	10,000
Fed at end of day .	1,339	13,348	124,995	722,173	990,042
Finally	9,746	89,606	496,032	907,771	989,994

These figures are for a *changing* population, provided that the total *number* of the population remains fixed (static); that is, the fact that

v is unity does not imply that every element in v is zero. The difference between the two second rows is due entirely to the higher birthrate of the mosquitos, which gives a modulus less than unity. The value of this modulus for the mosquitos is given in the third row of the mosquito table, since the final value, when t is increased indefinitely, is $B'_t = \mu' p'$. The modulus for the human population is more nearly unity.

If we wish to ascertain how many of the *original* population have been bitten or fed in one day we disregard the elements n, i, e, and retain only b and m (the mortality). That is, we have only to multiply the figures given in the human table by the factor $(1-m)^t$. For mosquitos the value of this for one day is estimated in (i) above as ·8959586; and as 1/3 for ten days. Thus out of a million mosquitos biting at a rate of 100 per minute, and dying at the rate of 76·6 per minute, 120,166 will be the number of surviving fed ones at the end of the day (the total number of survivors will be 895,959).

Of course it will be understood that most of the fed mosquitos or bitten persons at the end of the day will have been fed or bitten more than once during the day. The most probable proportions of those fed or bitten once, twice, thrice, etc., can be ascertained from (8) above, and will be studied in (15) below. The proportion of those which have never been fed or bitten dwindles as t increases, but never reaches absolute zero; and if the modulus is not unity it always approaches $1-\mu$, that is, $(1-b)n/(b+n)$ in value.

The bitings may be taken as applying to any kind of feeding, and therefore to male as well as to female mosquitos. But if the definition is confined to feeding on warm-blooded animals, the above table will apply only to the females—which may be taken as numbering half the total mosquito population. If it is confined to feeding on men only, the value of b' will be less than the most general one.

The question now remains, What is the most likely value of b' for human bitings? We have absolutely no experimental data to guide us; but may suppose, as a working hypothesis, that only about 1/8th of the surviving females will have succeeded in feeding at the end of 24 hours. This proportion is given nearly exactly (*vide* table) by $b' = {\cdot}0001$, or 100/1,000,000 bites in a minute. The static value is $B'_t = {\cdot}496\, p'$, or almost half the females in a static population; that is, if $b' = {\cdot}0001$, half the females always present are fed ones. Out of 1,000,000 female mosquitos, 1/27th, or 37,037 should survive for one month; and at the

above rate ·987 of these, or 36,543, will have been fed at least once during that time. This is probably a high, or possibly a maximum, standard if only bitings of men are being considered.

Since $bp = b'p'$, $b = b'p'/p = b'A$, where A is the proportion of female Anophelines to one human being. Thus, if $A = 10$ and $b' = \cdot 0001$, $b = \cdot 001$ a minute. With these constants, out of 10,000 female Anophelines and 1,000 persons, 1,249 of the former will have been fed, and 784 of the latter will have been bitten at the end of one day (*vide* table).

The total number of bites given in the day will be 1440 $b'p'$—see (8) above. Thus the *average* number for the whole female mosquito population will be $1440b'$; which amounts to ·144 with the standard value of b' just suggested.

Of course, the value of b' will be largely reduced if the people use mosquito nets or punkas; or if the weather is cold; or if the species of insect is not very voracious—as I have sometimes experienced.

From the equation $b'p' = bp$ we see that if p' and b remain constant, b' varies directly as p, the number of the human population—as, indeed, we should expect. Since we have supposed that a bite lasts for only one minute, each bite does not necessarily imply a *complete feeding*.

It is easy to calculate the total number of bites given during their lifetime by an original mosquito population subject only to deathrates. For if $b'p'_t$ be the number of bites given during the tth unit of time (which is a brief one), and if $p'_t = (1-m')^t p'_0$, then the total number of bites given by p_0 in their lifetime will be

$$\begin{aligned} \text{total bites} &= b'p'_0 + b'p'_1 + b'p'_2 + \dots \text{ ad inf.} \\ &= b'p'_0\{1 + (1-m') + (1-m')^2 + \dots \text{ ad inf.}\} \\ &= b'p'_0/m' \end{aligned}$$

If $b' = \cdot 0001$ and $m' = \cdot 0000766$ per minute, this becomes $1\cdot 305483\, p'_0$.

(iii). *The Infectivity Ratios.*—Owing to the fact that the plasmodia require 7-9 days under the most favourable circumstances to mature in the Anophelines, not all the bites given by infected insects will also be *infective*; and for similar reasons not all infected persons will be able to infect the mosquitos which bite them. In (11) above we used the symbols f' and f to denote the infectivity ratios. The value of f' may be estimated by several methods; but the following is probably the simplest.

Suppose that p'_0 is a large batch of Anophelines fed on an infective person at the same time, and then liberated and allowed to die at the

usual mortality rate of m' per minute; and assume that none of them become infective until the end of the tenth day—that is when $t = 14400$ minutes. Then the total number of bites given by them after that day will be

$$\begin{aligned}\text{infective bites} &= b'p'_{14400} + b'p'_{14401} + b'p'_{14402} + \ldots \; ad\ inf. \\ &= b'p'_0(1-m')^{14400}\{1 + (1-m') + (1-m')^2 + \ldots \; ad\ inf.\} \\ &= b'p'_0/3m'\end{aligned}$$

since by (1) above $(1-m')^{14400} = \frac{1}{3}$. But we have just estimated that the total bites given by an original mosquito population p_0 is $b'p'_0/m$. Hence the infective bites should be one-third the total bites; and as each mosquito bites only once in the small unit of time, the same fraction will represent the proportion of infective mosquitos at that time. That is $f' = 1/3$. This of course assumes that the mortality rate is as given in (1); and we may write more generally

$$f' = (1-m')^q$$

where q is the average time which elapses before an infected mosquito becomes infective. This period is certainly prolonged by cold, and possibly by the species of insect concerned.

We do not know with certainty how many infected persons are infective—though this could easily be ascertained by searching the blood of a sufficient number of patients for the gametids. I will therefore take $f = 1/4$, as in section 27 (where f was written i and f', s). This of course considers not only early cases of malaria but also chronic ones, in which the gametids are often absent or scarce for long periods. Moreover, these forms are not found at all until, say, 20 or more days after inoculation.

(iv). *The Naturally Non-immune Ratios.*—These are written g and g' in (11), and represent the proportions of men and Anophelines which are *naturally* resistant to the parasites—that is, are resistant without previous infection and recovery. I do not know that any men really possess this property, and therefore take $g = 1$. Regarding g', we know that a considerable proportion of mosquitos fed, or supposed to be fed, on an infective patient, are often found to be not infected; but this may frequently be due to their not having taken enough blood or to other causes. Pending more exact researches (which could easily be carried out), we may therefore take also $g' = 1$.

(v). *The Reversion Ratios.*—These are r and r'. Regarding the latter, I doubt whether any Anophelines which have once contained

protospores in their salivary glands ever cease to retain some of them and therefore to "recover" completely. Hence I think that we had better put $r' = 0$.

I have discussed the human recovery rate in sections 21 and 28, and suppose for a rough estimate that about one-half the patients remain infected after three months; that is, by (7, iv)

$$(1-r)^{1440\times 90} = \tfrac{1}{2}$$

From this we calculate that for one minute $1-r = \cdot 99999462$, and $r = \cdot 00000538$; and for one day $1-r = \cdot 9923280$, and $r = \cdot 0076720$.

Critics have questioned whether this rate is not too high, because, as stated in section 20, some cases may have relapses for years. But, of course, with the given rate 62·5 persons out of 1000 will remain infected at the end of one year, and 3·9 at the end of two, while a very small proportion may remain so for years. Unfortunately it is difficult to obtain more correct estimates, especially as we do not know the effects of reinfection.

I must now add an important definition of the word "recovery," as it must be used in this pathometrical work. As stated in (7, iv) above, it is a pathological element; but this does not mean that it is to be employed in the *medical* sense as recovery from *sickness*. Sickness is only an incident in the course of an infection, and we are here talking of *infections*. Still further, recovery here does not even mean the disappearance of the infective parasites from the body. It means *such loss of acquired immunity as renders the subject liable to reinfection however slight*. This is evident from the fundamental difference equations; and it is therefore all the more difficult to assign a strict value to the recovery ratio in the case of malaria. In most diseases immunity appears to outlast infection by considerable periods.

(13). *The Static Malaria Equation.*—Returning now to the static equation for insect-borne diseases, (ad) of subsection (11), we have only to substitute in it the constants as indicated above. Let $A = p'/p$—that is, let A be the proportion of Anophelines to one human being, and take

$$\begin{array}{ll} N = 0 & r' = 0 \\ g = 1 & g' = 1 \\ f = 1/4 & f' = 1/3 \\ r = \cdot 00000538 & N' = \cdot 0001016 \end{array}$$

and, of course, $b = Ab'$. Then we have, on reducing,

$$z\{(1-r)(1+N')fb'f'b'A + rb'f\} = p\{(1+N')fb'f'b'A - rN'\} \quad . \quad (af)$$

If we neglect the small quantities N' and r when bracketed with unity (as we may do for our present purpose), and put $B=\frac{N'}{b'f}$ and $C=\frac{r}{b'f'}$, we obtain

$$\frac{z}{p}=\frac{A-BC}{A+C}$$

$$A=\frac{p}{p-z}BC+\frac{z}{p-z}C \quad . \quad . \quad . \quad . \quad (ag)$$

Here z, the number of infected people, can have only positive values. It obviously becomes zero when $A=BC$, and after that increases continually, if either A, b', f, or f' increase, until it approaches p.

The value of A when $z=o$, that is $A=BC$, may be called its critical value, because it gives the maximum number of Anophelines per man which may be present *without* spreading malaria. With the constants suggested above $BC=\cdot 000{,}000{,}006{,}559{,}296/b'^2$; and is largely influenced by the biting factor. Thus, when $b'=\cdot 0001$ per minute, which I inferred in (12, ii) was a probable but high standard, the critical value of Anophelines per man is ·6559. But when $b'=000{,}01$ per minute, many more Anophelines, namely 65·59, are required to reach the malaria-producing point.

In the second equation given above, the term $\frac{z}{p-z}C$ is small compared with the first term, if b' is less than ·0001, especially if z is not high. We may then write for rough computation $A=\frac{p}{p-z}BC$ and $z/p=1-\frac{BC}{A}$. The latter is evidently the same function as the equation (7) of section 28 for static malaria; but we must observe that here A is the number of *female* Anophelines per man present at *any one moment*, and is not at all the same thing as a in section 28 and in Mr Waite's article, which denotes the number of *different Anophelines, male or female, per month.*

I now give the number of Anophelines per man calculated for various malaria rates and for various biting factors, with the constants suggested above (taking $1-r$ and $1+N'=1$).

	$b'=$ ·0001	·000,08	·000,06	·000,04	·000,02	·000,01
$z/p=0$;	$A<0{\cdot}6559$	1·0249	1·8220	4·0996	16 398	65·593
$z/p=\cdot 1$;	$A=0{\cdot}7467$	1·1612	2·0544	4·5999	18·310	73·038
$z/p=\cdot 2$;	$A=0{\cdot}8602$	1·3325	2·3448	5·2352	20·699	82·394

$z/p = \cdot 3$;	$A = 1\cdot 0062$	1·5506	2·7182	6·0294	23·772	94·396
$z/p = \cdot 4$;	$A = 1\cdot 2008$	1·8426	3·2160	7·1016	27·868	110·398
$z/p = \cdot 5$;	$A = 1\cdot 4732$	2·2315	3·9131	9·1026	33·603	132·800
$z/p = \cdot 6$;	$A = 1\cdot 8819$	2·8648	4·9586	10·8541	42·206	166·403
$z/p = \cdot 7$;	$A = 2\cdot 5630$	3·8870	6·7011	14·6067	56·444	222·409
$z/p = \cdot 8$;	$A = 3\cdot 9252$	5·9314	10·1861	22·1118	85·219	334·421
$z/p = \cdot 9$;	$A = 8\cdot 0119$	12·0646	20·6413	44·6271	171·245	670·456

The great effect of the biting factor is very apparent. As stated in (12, ii), this will vary with the species of Anopheline and certainly with temperature and humidity. I think that the dampness and darkness of dwellings may also affect it, though they may influence A too. Mosquito nets, wind, etc., may diminish it.

The rapid rise of the malaria rate, in consequence even of a small rise of the Anopheline rate above the critical number, is again most apparent. Thus, whatever the biting factor may be, the effect of doubling the critical number will be, roughly, to increase the malaria rate from zero to half the total population! And yet I doubt much whether this doubling could be ever detected without the most careful study. When we add to this the great effect of the biting factor, we shall understand why the correlation between the numbers of mosquitos and the amount of malaria has not always been immediately apparent.

These laws are, I think, not second in importance to any law connected with our subject. They apply also to other metaxenous diseases, and, I may point out, could not have been obtained at all except by pathometrical methods. They must form the basis of the reduction of these diseases by the reduction of the alternative hosts, because they show that *ceteris paribus* a large reduction of the former is likely to result from even a small reduction of the latter—certainly if the latter were originally not more than twice the critical number.

The recovery, or rather *reversion*, factor, r, was not exactly defined in section 28; and with the necessary definition of (12, v) above seems to lose some of its value. On the other hand, the infectivity factors, f and f', are very important. The public-health effect of quinine must be mainly exerted through f in diminishing the infectivity by reducing the gametids in man; and f' must be greatly influenced by the death rate of the insects as well as by their species. Thus the malaria would be diminished by increasing either the birthrate or deathrate of the mosquitos. It would, therefore, be still more diminished by increasing both together; and yet such simultaneous increase might leave the

total mosquito population, A, the same as before!—another law which may help to explain several phenomena.

It will now be advisable to compare briefly the value of A found here with that of a found in section 28. The former consists of females found at any moment; the latter of different males and females during one month. The number of different individuals found during a month will be composed of the original population, p_o, plus the births during the month—excluding the immigrants. That is, if the population remains fixed in number, it will be $p_o + 43200np_o$—taking n as the birthrate (·0001016) per minute.[1] Thus the different individuals will number $(1+4{\cdot}3891)p_o$ or $5{\cdot}39p_o$ during the month, where p_o is the number found at any one moment. If p_o consists entirely of females we must multiply it by $2 \times 5{\cdot}39$ to obtain both males and females during the month—that is, by 10·78. In section 28, about 40 different male and female Anophelines per month were taken as the critical number when the malaria rate is zero. This would therefore correspond in the above table to a biting factor of a little more than ·000,04 per minute; and we must apply the same factor to the other figures in the column to reduce to the standard of section 28. (A very rough method of dealing with this problem is given in section 29(4), but I have no space for full treatment of it.)

(14). *Happenings of Infinitesimal Period.*—In the above I have supposed the unit of time during which h individuals are affected by any happening to be a finite unit, such as a minute or a day—though we may make that unit as small as we please; and this is the most general way of treating the subject. But it will be advisable to add now some notes on the case in which the unit of time is taken as being infinitesimally small. This of course brings the subject under the Infinitesimal Calculus, and is useful because it enables us to consider *continuous* variation in the functions.

(i). Let dt be the infinitesimally small unit of time. Then the various elements will also be infinitesimally small, and may be written $h.dt$, $r.dt$, $n.dt$, $N\,dt$, etc.; and their products, being of the second order of smallness, may be neglected. We thus have

$$v = 1 + (n - m + i - e)dt$$
$$V = 1 + (N - M + I - E)dt$$
$$hdt.v = h.dt$$
$$(1 - hdt)v = 1 - hdt + (n - m + i - e)dt$$

[1] $43200 = 03 \times 24 \times 60$.

$$H=(1-hdt)(r+N)dt/(1+Ndt)$$
$$=(r+N)dt$$
$$(1-H)V=1-(r+N)dt+(N-M+I-E)dt$$

Thus the fundamental equations of (6) above, namely

$$a_{t+1}=(1-h)va_t+HVz_t$$
$$z_{t+1}=\qquad hva_t+(1-H)Vz_t$$

should now be written

$$a_{t+dt}-a_t=\{(v-1)-h\}dt.a_t+(r+N)dt.z_t$$
$$z_{t+dt}-z_t=hdt.a_t+\{(V-1)-(r+N)\}dt.z_t \quad . \quad . \quad (ah)$$

that is, $$\frac{da}{dt}=Pa+Qz \quad \frac{dz}{dt}=P'a+Q'z$$

where P, Q, P', and Q' are the coefficients. Differentiating and eliminating, we have

$$\frac{d^2a}{dt^2}=P\frac{da}{dt}+Q\frac{dz}{dt}=P\frac{da}{dt}+QP'a+Q'\left(\frac{da}{dt}-Pa\right)$$

that is, $$\frac{d^2a}{dt}-(P+Q')\frac{da}{dt}+(PQ'-P'Q)a=0 \quad . \quad . \quad . \quad (ai)$$

with the same equation for z. Putting, in the latter, $z=\epsilon^{xt}$, we obtain for the auxiliary equation

$$x^2-(P+Q')x+(PQ'-P'Q)=0$$

Replacing P, Q, P', Q', by their original values and rearranging,

$$x^2-\{(v-1)+(V-1)-(h+r+N)\}x+(v-1)\{(V-1)-(r+n)\}$$
$$-(V-1)h=0$$

As in (7 (g)), the roots of this are rational if $v-1=V-1$; and are then $(v-1)$ and $(v-1)-(h+r+N)$. Denoting these by X and Y, we have

$$a=c\epsilon^{Xt}+C\epsilon^{Yt}$$
$$z=c'\epsilon^{Xt}+C'\epsilon^{Yt}$$

The four constants are determined easily by putting $t=0$ in these and in their differential coefficients, and comparing with the original differential equations: whence,

$$(h+r+N)a=(r+N)(a_0+z_0)\epsilon^{Xt}+\{ha_0-(r+N)z_0\}\epsilon^{Yt}$$
$$(h+r+N)z=\qquad h(a_0+z_0)\epsilon^{Xt}-\{ha_0-(r+N)z_0\}e^{Yt} \quad . \quad (aj)$$

which are the same as the equations of (7, ii), and may be written in the various forms there indicated.

(ii). If in the second equation (ah) we put $p-z$ for a, we have

$$\frac{dz}{dt}=h(p-z)+(V-1-r-N)z \quad . \quad . \quad . \quad (ak)$$

This is the infinitesimal form of equation (z) above. In it, the total population, p, may be a variable function of t; but if we take it as remaining constant during the enquiry, we obtain an equation which is immediately integrable in the reciprocal form for $\frac{dt}{dz}$. The integration gives, if $Y=V-1-h-r-N$,

$$Y(t+C)=log(hp+Yz)$$

where YC is the value of $log(hp+Yz_0)$ when $t=0$. Hence

$$Yz+hp=(Yz_0+hp_0)\epsilon^{Yt} \quad . \quad . \quad . \quad . \quad (al)$$

If $V=1$, this is the same as equation (aj) just given, and furnishes the value of z when the happening coefficient, h, is a *constant*.

(iii). In many infectious diseases, however, the infection-rate, h, is likely to vary directly as the number of infected persons present at a given moment. Suppose therefore that $h=cz$, where c is some constant coefficient; then, if p is constant,

$$\frac{dt}{dz}=\{cz(p-z)+(V-1-r-N)z\}^{-1} \quad . \quad . \quad . \quad (am)$$

Hence if $Q=V-1-r-N$ (which is negative if $V=1$),

$$(cp+Q)t=log\frac{cz_0-cp-Q}{cz-cp-Q}\frac{z}{z_0}$$

or

$$z\left\{1+\frac{c(p-z_0)+Q}{cz_0\epsilon^{(cp+Q)t}}\right\}=\frac{cp+Q}{c} \quad . \quad . \quad . \quad (an)$$

As $cp+Q$ must be positive, z reaches the limit $\frac{cp+Q}{c}$ when t is indefinitely increased.

This case would include equation (4) of section 28, in which we supposed $h=b^2saimp$ (where mp is now expressed by z), and if the constants are suitably taken the formula would represent the continuous change in the malaria rate on the supposition that h does really equal cz. This was, however, only an approximate supposition required for a simple statement of the subject.

(iv). A more correct determination of h for the metaxenous diseases must be made by the method given in (10) above. If z

and z' are the number of affected individuals of the two kinds of hosts respectively, then we shall have the two following simultaneous equations to solve,

$$\frac{dz}{dt}=k'z'(p-z)+(V-1-r-N)z$$

$$\frac{dz'}{dt}=kz(p'-z')+(V'-1-r'-N')z' \quad . \quad . \quad . \quad (ao)$$

The values of z and z' become static when both $\frac{dz}{dt}$ and $\frac{dz'}{dt}$ become zero. In this case we shall have

$$z=\frac{kpk'p'-QQ'}{k(k'p'-Q)} \quad z'=\frac{k'p'kp-Q'Q}{k'(kp-Q')} \quad . \quad . \quad . \quad (ap)$$

which are the same as the equations reached in (10) and (11) above. Here then, $k'z'$ and kz, that is, h and h', must be constant—the case becomes that of (ii) above, and k' and k will have, for malaria, the values suggested in subsection (11). But when the differential coefficients are not zero, z and z' will be continuously changing. Hence the general solution is more complicated than that of the special static case, and cannot be dealt with here.

(15). *Repeated Bitings of Insects.*—The theory of repeated happenings given in subsection (8) may be usefully employed in several ways—as in the two following examples. According to that calculation, if h is the proportion of a population, p, affected by any kind of happening in a very short unit of time, then the most probable proportion of individuals to which the happening has occurred no-times, once, twice, thrice . . . . in the period t will be given by $p\epsilon^{-ht}$ multiplied by the successive terms of the expansion of ϵ^{ht}, that is by 1, ht, $\frac{1}{2}h^2t^2$, $\frac{1}{2\,3}h^3t^3$, etc. The numbers must of course depend on the values of h and of t; but we can estimate them very readily if we take t such that $ht=1$. In this case we have merely to multiply $p\epsilon^{-1}$ successively by 1, 1, $\frac{1}{2}$, $\frac{1}{6}$, $\frac{1}{24}$ . . . .; or, what is the same thing, divide each previous term successively by 1, 1, 2, 3, 4. . . . . Thus, since $\epsilon^{-1}=\cdot367879$, if $p=1{,}000{,}000$, $p\epsilon^{-1}=367{,}879$—which is the number of individuals to which the happening has not occurred at all during the period $t=1/h$. The number of individuals to which the happening has occurred only once will be the same; to half as many the event will have occurred twice; and so on.

Suppose that in an original population of 1,000,000 mosquitos

100 succeed in *feeding* every minute. Then h (or b') = ·0001 per minute. Now take t such that $t = 1/h$; that is, we take a period of 10,000 minutes (= 166·6 hours, or nearly one week). Then, if all the insects survive for this period, the numbers fed x times should be as follows:

Times fed	0	1	2	3			total
Number	367,879	367,879	183,940	61,313			999,999
Times fed	4	5	6	7	8	9	
Number	15,328	3,066	511	73	9	1	

So that as many should have fed only once as not at all, and only one should have succeeded in feeding nine times.

If, however, we suppose that the insects are subject to their ordinary death rate, then we must multiply each term by $(1-m)^{10,000}$ (or $\epsilon^{-10,000m}$), where m is the mortality per minute. The result will give the number of *survivors* at the end of the week which have fed x times.

This, of course, refers to special feedings, such as those on man. Otherwise the mortality of the unfed insects would doubtless be much higher than that of the fed ones, and the proportions would have to be calculated from the general happening equations in the case where v does *not* equal V.

Suppose that the feeding factor per minute is only ·00,001. Then the same proportions would be reached in nearly ten weeks. To find the proportions at the end of one week we must divide $\epsilon^{-\cdot 1}$, or ·904837, successively by 1, 10, 20, 30, ... Thus only ·090484 of the insects will now have fed once, and ·004524 twice.

It is convenient to use the name *standard periods* for the values of t which make $\epsilon^{ct} = 1$ or $\epsilon^{-ct} = \epsilon^{-1}$, where c is any number employed in these calculations—see (17) below.

If the feeding factor is ·0001 per minute, 1,000,000 mosquitos will have 1,000,000 feeds in the standard period—if they all survive; that is, an average of one each. But only 632,121 will have fed at all; so that on the average each of *these* will have had 1·582 feeds in the week.

(16). *Repeated Infections.*—Obviously in any malarious place a number of persons will be infected over and over again; though, if they are already infected they will not be able to recognise when such reinfections occur. The theory of repeated happenings enables us to calculate by precisely the same method just used how many persons

are infected no-times, once, twice, etc., in a given period. Suppose that the malaria in a place is static. Then

$$z = \frac{h}{h+r+N}p \qquad\qquad h = \frac{z}{p-z}(r+N)$$

From this we can calculate h, and therefore also the standard period, $t = 1/h$. Then in that period the numbers of persons infected no-times, once, twice, etc., will be the same as those just given for repeated feedings during a standard period.

For example, let half the people be affected; so that $z = \frac{1}{2}p$. Let $r = \cdot000,005,38$ per minute as in (13) above, and let N, the birthrate per minute, be ·000,000,004, 75. Then $h = \cdot000,005,384,75$ per minute, and the standard period is $1/h = 185,710$ minutes, or 129 days. Hence, during this period, out of 1,000 people 368 will not have received any infection, and 368, 184, 61, 15, and 3 will have been infected respectively 1, 2, 3, 4 and 5 times. The total number receiving infection during the 129 days will be 632, and each of these will have been infected 1·582 times on the average.

This may appear incongruous with our assumption that the static malaria rate was one-half, or 500 out of the 1000 people—because here we find that no less than 632 persons have been infected in a little over four months. But we must remember that the reversion rate is calculated on the basis that half the sick revert every three months. In the face of this reversion rate, the infection rate must be very high to maintain the malaria rate at so high a figure as one-half.

If the malaria rate is only one-quarter, we should have h one-third of the value just given. Hence the standard period would be three times as large namely 387 days (provided that the malaria constants remain unchanged during all the period). But at the end of this larger period the number of persons infected no-times, once, twice, etc., would be the same. If the malaria rate is only one-tenth of the population, the standard period would be nine times larger than when it is one-half—that is, it would be 1161 days. During one-tenth of this period, namely 116 days, 905 people out of the thousand would remain unaffected—that is, $\epsilon^{-1/10} \times 1000$.

(17). *Some Values of* ϵ^{x} *and* ϵ^{-x}.—Readers who may wish to follow these calculations but who possess no book of logarithms may like to have a simple table for dealing with the exponential functions of time used in this section. Of course if x is not greater than unity, and especially if it is much smaller, we can easily obtain approximate values

from the expansion $\epsilon^x = 1 + x + \frac{1}{2}x^2 + \frac{1}{2 \cdot 3}x^3 + \frac{1}{2 \cdot 3 \cdot 4}x^4 \ldots$ (if x is negative the second, fourth, sixth terms in the expansion will also be negative). But we can often follow the simple course of taking t such that x has one of the values given in the following table:

$x =$	0·1	0·2	0·3	0·4	0·5
$\epsilon^x =$	1·105171	1·221403	1·349859	1·491825	1·648721
$\epsilon^{-x} =$	·904837	·818731	·740818	·670320	·606530
$x =$	0·6	0·7	0·8	0·9	1·0
$\epsilon^x =$	1·822119	2·013752	2·225541	2·459603	2·718282
$\epsilon^{-x} =$	·548811	·496585	·449329	·406569	·367879

(18). *Affectedness, Infectedness, Infectivity, and Illness.*—All these are evidently different. The symbol z denotes all the individuals which at the moment of enquiry are actually *affected* in any way by the happening. In disease this covers the whole period from the first moment of inoculation until the patient has passed so completely out of the influence of the happening that he is capable of reinoculation (for example)—that is, until he has lost his acquired immunity, if any. But *infectedness* covers only the period during which the infection is actually living in his body; that is, the period from inoculation until the parasites die out completely. Since immunity often or generally outlasts the presence of the infection, infectedness will generally cover only a first portion of the period covered by affectedness. Similarly, *infectivity* covers only a period, often short, during which the infecting organisms are capable of passing from a patient to another person; and, lastly, *illness* covers merely the portion or portions of the period of affectedness during which the patient is sick—a very indefinite adjective. Hence infectedness, infectivity, and illness are all of them only *episodes* in the course of affectedness—beginning on a certain date and ending on a later date, both dates being within the duration of the last named condition. As the above calculations have hitherto referred only to z, the number of affected individuals, it now becomes important to ascertain what proportion of these will be, at any moment, also in the condition of one of these "episodes."

The subject is capable of elaborate examination; but it will suffice to use here the simple process employed in (12, iii) for determining the infectivity ratio of Anophelines in malaria. If we can ascertain the total number of days (or other units of time) lived by an affected population, and also the number of days lived during the episode under study, we can then obtain the ratio of the latter to the former; and the

ratio of the number of individuals "in episode" to the total number affected will be the same.

The number of days lived by a population during a given period is evidently the product of the number of days during the period and the sum of the varying or constant populations living on each day: if there are 1,000 individuals alive on each day, 10,000 days will have been lived in 10 days. Now let p_0 be an original population, all of whom have been infected at the same moment and are thereafter subjected to the usual reversion and mortality rates. Then if $p_1, p_2, p_3 \cdots p_t$ be the number of them living at the end of the first, second, . . . tth day, the number of days lived by them up to t will be $p_0+p_1+p_2 \cdots p_{t-1}$. The value of p_t (or z_t) is given in (7, iv) above, and is $(1-r)^t(1-M)^t$ if we retain only these elements; and the sum of $p_1+p_2+ \ldots$ is found by integrating this function. If r and M are small the general integral may be written $-\frac{(1-r-M)^t}{r+M}$; or, in the exponential form, $-\epsilon^{-(r+M)t}/(r+M)$. Hence, if we wish to obtain the sum of the populations between the two periods t_1 and t_2, we have

$$\sum_{t_1}^{t_2}(1-r-M)^t p_0 = \frac{p_0}{r+M}\{(1-r-M)^{t_1}-(1-r-M)^{t_2}\}$$

or

$$\int_{t_1}^{t_2} \epsilon^{-(r+m)t} \cdot p_0 = \frac{p_0}{r+M}\{\epsilon^{-(r+M)t_1}-\epsilon^{-(r+m)t_2}\} \quad . \quad . \quad . \quad (ag)$$

And these give the number of days lived between t_1 and t_2. If $t_1=0$ and $t_2=\infty$, both expressions become $\frac{p_0}{r+M}$; so that this gives the total number of days lived by the whole affected population p_0 while they remain affected and survive. Otherwise, if t_1 is the date of the beginning of an episode and t_2 that of the end of it, the definite integral will give the number of days lived by the population while they are passing through the episode—that is, while they are infected, infective, or sick. And the ratio of this number to the total affected days $\frac{p_0}{r+M}$ gives the proportion of episode-days to affected-days. Finally we infer that in a general population of individuals infected on different days the proportion of episode-persons to total affected persons will be the same; that is, will be

$$(1-r-M)^{t_1}-(1-r-M)^{t_2}$$

or

$$(t_2-t_1)(r+M)-\tfrac{1}{2}(t_2^2-t_1^2)(r+M)^2+\tfrac{1}{6}(t_2^3-t_1^3)(r+M)^3 \quad . \quad (ar)$$

approximately.

For example, suppose that we wish to know the probable proportion of persons affected with malaria who are also in the acute stage of fever, which, say, lasts from the fifteenth to the thirtieth day after inoculation. Taking $r = \cdot 0076720$ *per diem* and $M = \cdot 0000675$ *per diem* (which $= \cdot 024349$ *per annum*, a low figure for malaria), we have $(\cdot 99336 05)^{15} - (\cdot 9933605)^{30} = \cdot 086051$. That is, the persons with acute primary fever number about one-twelfth of the total affected persons (z).

The average number of affected days lived by a person in the case of malaria with the constants used above would be 129, or nearly the same as the *standard period* obtained in (16) above for the case when $z = \frac{1}{2}p$. This is because the birth- and death-rates are taken as nearly equal. But the malarial death-rate is probably considerably higher, and the affected days lived $(1/(r+M))$ fewer.

Of course, in the applications to various diseases, the constants must be separately calculated—and we often possess very poor data for the work. The mortality presents this difficulty, that it is not constant, but generally much higher during the first, acute stage of the affectedness. We then proceed as follows. We first divide the whole affected period into three, the incubation period, the acute period, and the subsequent period. For the first and third we suppose that the ordinary death-rate, m, holds, and for the second period, the morbid death-rate M; and we then calculate separately the total affected-days and the episode-days by means of the formula. But the ratio is chiefly determined by the length of the episode-period in comparison to the total affected-period.

By putting $r = o$, $t_1 = o$ and $t_2 = \infty$ we find the total average number of days lived by the whole population, with the given death-rate. Thus with the rate used above it amounts to nearly 40·6 years.

(19). *Conclusion.*—I cannot supply in time for the present edition of this book any fully worked-out examples of the applications of the theorem to diseases other than malaria—but such applications are numerous and important. For example, it has recently been observed that sleeping sickness smoulders in the Gambia hinterland, giving a value of z less than 1%; and it has been suggested that this may be due to racial immunity of the people, that is, to a low factor, g, in equation (ad). But another sufficient explanation might be simply that the *Glossina palpalis* is not numerous enough over and above the critical point to cause more widespread disease. This is likely to be the case because, I hear, z is highest in proximity to water—that is,

just where, on the other hypothesis, g and z should be lowest. Another important case is that of yellow fever. When the disease was shown to be carried by mosquitos, it was at once assumed to have laws of endemicity similar to those of malaria, namely that the native human population were the reservoirs of the infective organism, though they had acquired immunity to its effects. But a study of equation (*ad*) shows that this cannot be the case, if certain classical observations are sound. Immunity is so lasting in this disease that r may be put almost at zero, so that the affected-days continue almost to the average life of the population. On the other hand good experiments have nearly proved that the human host is infective to mosquitos for only about three days. That is, the ratio of infective to affected days is extremely small; so that the factor f of equation (*ad*) is also extremely small. Moreover, so far as we can judge by the loss of infectivity and illness the infected-days are also very few—say one week or so. On the other hand *S. calopus* lives for months and remains infective all the time; so that its infectivity factor f' and also its infected ratio are very high. From these data it is clear that yellow fever must be of very low endemicity in man (endemicity = the infected ratio), and of very high endemicity in the mosquito. In other words, the mosquitos and not the men are the "reservoirs" of the infection. It has also been assumed that where the disease smoulders, this is because the human z is high—because all the natives have acquired immunity and only new-comers are infected. But a low Stegomyia factor will easily explain the same thing. Much has been written regarding the absence or presence of yellow fever in the Old World. The disease is certainly absent in many countries where *S. calopus* exists. But Mr F. V. Theobald informs me that, from his study of large collections, this insect appears to be much more rare in the Old World than in the New; and this, if correct, might be a quite sufficient explanation of the whole phenomenon.

The venereal diseases may be looked upon as metaxenous diseases in which the two sexes take the part of the two hosts. Thus the constants of the two groups will be the same, and $z = z'$; so that these diseases come under the case of equation (*an*).

The reader should examine as an exercise the equations for the number of affected Anophelines in malaria. It will be seen that the two terms of the second equation (*ag*) give respectively the proportions of healthy and of affected mosquitos to each man; and that the

malaria rate among the insects is always much lower among the insects than among the human population. Even if all the latter are affected, and if the biting factor is as high as ·0001, only about one-fifth of the female Anophelines will be infected ones (the other constants being assumed correct).

The important phenomenon of what may be called "smouldering disease" occurs when p' remains just at the critical point; and the reader should consider carefully the conditions connected with it.

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